

SCIENCE

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FRIDAY, APRIL 21, 1899.

CONTENTS:

<i>Othniel Charles Marsh (with Portrait):</i> DR. J. L. WORTMAN	561
<i>Some Misapprehensions as to the Simplified Nomenclature of Anatomy:</i> PROFESSOR BURT G. WILDER	566
<i>The Breeding of Animals at Woods Hole during the Month of September, 1898:</i> M. T. THOMPSON...	581
<i>Economics in Manufactures:</i> PROFESSOR R. H. THURSTON	583
<i>Scientific Books:—</i>	
<i>Harkness and Morley on Analytic Functions:</i> PROFESSOR JAMES PIERPONT. <i>Schnabel's Handbook of Metallurgy:</i> DR. J. STRUTHERS. <i>Books received</i>	586
<i>Scientific Journals and Articles</i>	589
<i>Societies and Academies:—</i>	
<i>The Anthropological Society of Washington:</i> DR. J. H. McCORMICK. <i>Geological Conference and Students' Club of Harvard University:</i> J. M. BOUTWELL. <i>The Torrey Botanical Club:</i> EDWARD S. BURGESS.....	590
<i>Discussion and Correspondence:—</i>	
<i>Duplication of Geologic Formation Names:</i> DR. GEORGE M. DAWSON. <i>On the Names of Certain North American Fossil Vertebrates:</i> O. P. HAY. <i>The Fundamental Law of Temperature for Gaseous Celestial Bodies:</i> PROFESSOR W. S. FRANKLIN.....	592
<i>Notes on Inorganic Chemistry:</i> J. L. H.....	595
<i>The Naples Zoological Station</i>	596
<i>Scientific Notes and News</i>	597
<i>University and Educational News</i>	600

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OTHNIEL CHARLES MARSH.

THE last of the famous trio of American vertebrate paleontologists has passed into the unknown, and the rich legacy of discovery and advancement in biological knowledge which they have bequeathed to the world will ever stand as an enduring monument to their untiring energy and greatness in the realm of thought. It seems, therefore, especially fitting that the unveiling of this splendid monument and the final pronouncing of judgment upon the labors of these truly great Americans should take place in the closing years of the century, notable alike for the variety and brilliancy of its achievements in almost every department of learning.

At the time when the doctrine of Evolution was finally formulated and brought prominently before the thinking world by the labors of Darwin the direct and positive evidence in favor of such an hypothesis was inconclusive and uncertain. True, it received more or less powerful support from Mr. Darwin's own particular field of research, as well as from the embryological studies which the Germans had brought into especial prominence, but the court of the last resort, the tribunal of final judgment in which the case was to be argued and decided was that of the Geological Record, or, in other words, a direct appeal to the animals and plants themselves, which had inhabited the earth in times

past, and whose remains lie entombed in the rocks, mute but unimpeachable witnesses of the story of their becoming and development. It was generally agreed and fully admitted by the foremost thinkers of this critical period that these remains not only once formed parts of living animals, but that they furnish safe guides for the determination of the deposits in which they are found, in the general time scale of the earth's history.

Fossils representing the higher forms were not unknown in Europe at the time this discussion arose, but the specimens from which they were known were in general so fragmentary and lacking in consecutiveness as to furnish little evidence for or against the pretensions of the Darwinian hypothesis. To such an extent was this true that Darwin was compelled to add a chapter in his great work on the Origin of Species, on what he was pleased to call the 'Imperfections of the Geological Record.'

It was at this juncture or shortly afterward that the famous American trio appeared upon the scene, and the tremendous weight of their testimony derived from the unrivaled record of the fossil deposits of Western America has served to take the whole question practically out of the realm of discussion and reduce it to the plane of a demonstrated fact. It has been very truly said that if we regard the truth of Evolution from Mr. Darwin's especial point of view, viz.: that of living plants and animals, we shall conclude that it is a possibility; if we look at it from the standpoint of embryology our judgment must be that it is a probability, but if we examine it from the evidence of paleontology it is no longer a possibility or a probability, but a living truth.

Such, in brief, is the basis of the claims to distinction which the works of these men offer. The share which Leidy took in the

performance of this great work has already been told; the second chapter, devoted to the brilliant discoveries of Cope, has likewise been written, and it remains now to speak of the work of the man whose scientific labors form the subject of the present sketch.

Othniel Charles Marsh was by nature a student and early gave evidence of what his future career was to be by a love for nature and natural objects. As a boy he collected birds, insects, minerals and fossils. He was born in Lockport, N. Y., October 29, 1831, and in 1852 went to Phillips Andover Academy, where he graduated with honors. He afterwards entered Yale, from which institution he graduated in 1860. While in college he became deeply interested in geology, paleontology and mineralogy, and spent two additional years after his graduation in the Sheffield Scientific School at Yale and three years in Germany in pursuit of these branches. In 1866 a professorship of vertebrate paleontology was established in Yale and he was called to fill it. Between this and the time of his graduation he had published a number of important papers on 'Minerals and Fossils,' many of which appeared in the *American Journal of Science*. In 1868 he began his investigations of the Western fossil deposits, and this he was all the better able to do on account of the inheritance of a considerable fortune from his uncle, George Peabody, the banker. It was largely through his influence that this latter gentleman was induced to make the munificent gifts to the University which led to the establishment of the Peabody Museum at Yale.

The record of his discoveries from the time of his appointment to the professorship is one of almost continual triumph in the bringing to light of new and strange forms of life that had inhabited the western hemisphere in the distant past. Pre-

vious to the publication of any of his Western material he contributed some important papers upon the fossil birds and reptiles from the Cretaceous of the East. In 1869 appeared 'Notice of some New Mosasauroid Reptiles from the Greensand of New Jersey,' 'Description of a New Gigantic Fossil Serpent (*Dinophis grandis*) from the Tertiary of New Jersey' and 'Notice of some Fossil Birds from the Cretaceous and Tertiary Formations of the United States.' In 1871 he gave a description of his trip through the Uinta Mountains and the Discovery of the Uinta Tertiary Formation, the uppermost member of the Eocene series. In May of this year he published a description of some new fossil serpents from the Tertiary deposits of Wyoming, and in June of the same year he gave notice of the discovery of the first remains of Pterodactyles that had ever been found in America. In July of the same year he also published the first notice of Tertiary Mammals from the Western beds. In the following year, 1872, he was very active, and some of the most important discoveries of the long list to his credit followed in rapid succession.

It is quite impossible to give more than a brief list of his remarkable finds published during this and the succeeding years; the more important only must suffice. It appears astonishing, however, in the light of our present knowledge of the subject what a keen insight into their meaning and importance he possessed and of which he gave such distinctive evidence in his descriptions. In this year (1872) he proved beyond all doubt the existence of the Pterodactyles in this country, a group which hitherto had been regarded as entirely wanting in the western hemisphere; he described the first remains of the now famous toothed bird *Hesperornis*, although at this time, May, 1872, he did not know of its having possessed teeth. Among the Mosasauroid Reptiles he determined for the first time the following important points

in their structure: (1) position of the quadrate bone, (2) presence of the stapes, (3) presence of the collumella, (4) presence of the quadratoparietal arch, (5) presence of the malar arch, (6) the nature of the pterotic bone, (7) nature of the anterior limbs, (8) presence and nature of the posterior limbs and pelvis and (9) the number of the cervical vertebræ. He also announced in September of this year the discovery of *Ichthyornis*, the curious Cretaceous bird with biconcave vertebræ. Just previous to this came a long list of new genera and species of fossil mammalia from the Bridger Eocene horizon of Wyoming, which, although briefly described, are of the most intense interest and the highest importance in tracing the ancestry of many living mammalian groups. One of the most important of these discoveries among the fossil mammals was the demonstration of the existence of Lemurs, or Primitive Primates, on this continent.

Of scarcely less importance were his contributions of the following year; early in February, 1873, he announced the discovery of teeth, in both jaws, of *Ichthyornis dispar* and established for it, on this account, a sub-class, Odontornithes. This discovery was of far-reaching importance and satisfactorily established the fact that many of the Cretaceous birds are transitional between living birds and reptiles. In this year he devoted much time and space to the consideration of the gigantic mammals of the Eocene, of which the first notices had been given by Leidy from a few fragmentary remains. To Marsh, however, belongs the credit of the final determination of their structure and affinities; he classified them in a separate and distinct order, Dinocerata, a name which has been very widely adopted by naturalists.

In March, 1874, came the discovery which has tended to give Professor Marsh a greater reputation than any other single piece of

work in his entire career. Various futile attempts had been previously made to trace the ancestry of the Modern Horse. Huxley and Kowalewsky in Europe had established the fact that mammals belonging to the equine stem were found in Europe in the early Pliocene and late Miocene, but their attempt to trace the line into any older formations signally failed. Shortly after this Professor Marsh pointed out the equine nature of his Bridger genus *Orohippus*, and was the first to show that the fossil forms of the American Continent furnished every conceivable link between the small polydactyle species of the Eocene and the modern horse. So strong, indeed, is the evidence of this descent that were there no other evidences of evolution to be found among the fossils this would be quite sufficient of itself to establish its truth. In May of this year he published an important paper setting forth these discoveries on the 'Fossil Horses in America.'

In 1875 he published additional discoveries among the Cretaceous birds, and determined for the first time that *Hesperornis* possessed teeth in both jaws. In the succeeding year a series of important papers appeared, giving the principal characters of the *Dinocerata*, *Tillodontia*, *Brontotheridæ* and *Coryphodontia*. Of this latter group he was the first to point out that they were very closely allied to a genus that was described by Owen as early as 1846 from a few fragmentary remains found in the Eocene of Europe, thus giving the first secure basis for a comparison of the older Eocene deposits of the two countries. In this year he was elected Vice-President of the American Association for the Advancement of Science, and in the following year succeeded to the Presidency of the body. His address as the Vice-President upon the 'Introduction and Succession of Vertebrate Life in America' is a notable production and shows the wonderful knowledge he

possessed of the organization of the Vertebrates.

Some notable discoveries which marked the beginning of his extensive and important contributions to the knowledge of the extinct reptiles of the group *Dinosauria* from the Rocky Mountain region were published early in 1877. From this time on, almost up to the time of his death, one discovery after another pertaining to these weird gigantic creatures followed in rapid succession. This subject came to engross his attention more and more, and at the time of his death was the one in which he was the most deeply interested. In 1879 the first discovery of fossil Mammals from the Western Mesozoic was announced, and within the next few years a large number of genera and species were added to the list. His contributions to the subject constitute practically all we know of the American Jurassic Mammalia. In 1880 appeared his first important Monograph on the 'Extinct Toothed Birds of North America,' an important and beautifully illustrated volume published by the United States Geological Survey. In 1886 followed his second Monograph on the 'Dinocerata, An Extinct Order of Gigantic Mammals,' which served to bring together and present in extended form his many discoveries on this subject, a work which was likewise published by the Government Survey. In 1889 two discoveries of more than usual importance were made; one was the finding of a very extensive Cretaceous Mammalian fauna in the Laramie Beds of Wyoming, and the other the discovery of those curious horned Dinosaurs, the *Ceratopsia*, in the same deposits.

It would be impossible to give here even a list of his papers which have contributed so immensely to our knowledge of the extinct Reptilia. It is in this difficult group especially that his splendid knowledge will be so sadly missed, and it will, indeed, be

many years before any of the younger generation of paleontologists who survive him can hope to acquire the information of these various groups which he possessed. It was his intention and special desire to embody this knowledge in separate monographs, to be published by the Geological Survey, several of which were in an advanced state of completion at the time of his death. He had also projected extended works upon other groups. The volumes which he had mapped out and already done a considerable amount of work upon were as follows: The Sauropoda, Theropoda and Ornithopoda, to be in three separate volumes representing the three great divisions of the Dinosauria. Last year the Geological Survey issued a preliminary volume from him on the North American Dinosaurs. He also had a volume projected upon the Mesozoic Mammalia and one upon the Bronthotheridæ.

The scientific world at large had a just appreciation of his merits, and he was largely rewarded by many marks of distinguished consideration. He was elected a member of nearly every scientific society of note in Europe and America. In 1875 he was elected Vice-President of the American Association for the Advancement of Science, and in the year following he became President. In 1877 he received the Bigsby Medal from the Geological Society of London for the most distinguished researches in geology and paleontology. In 1882 he was chosen President of the National Academy of Sciences, a position which he held for two terms of six years each. In the same year he was chosen Paleontologist of the U. S. Geological Survey, a position which he held for ten years. He was also made honorary Curator of Paleontology in the U. S. National Museum, and held this position at his death. In 1886 the University of Heidelberg conferred on him the degree of Ph.D., and in the same year Harvard gave him an

LL.D. Last year he was made a corresponding member of the French Academy, and later he was announced as the winner of the Cuvier prize, one of the most distinguished honors ever conferred upon an American professor.

In his younger days he was a man of tremendous energy and spent much of his time in the field exploring for fossils, frequently far from the outposts of civilization. These expeditions were often attended with many hardships, and at times no small amount of risk to his personal safety, but wherever a new field offered opportunities for adding something novel, calculated to advance the knowledge of his science, no expense, hardship or danger could deter him from undertaking its exploration. The methods of collecting and preparing these fossils for study and exhibition which he has introduced in the course of his long experience forms the basis very largely of all similar work in almost every paleontological laboratory of the world, and it is a matter of common remark that nearly all the noted collectors and preparateurs have received their training under his immediate influence.

The vast collections on this subject which he has brought together are without doubt the finest and most complete of any in the world, and, when properly installed and exhibited, will make a monument in every way worthy of the greatness of the man who dedicated his life and his fortune to its formation. The influence of his work for advancement in this department of knowledge has probably had no equal in any country, and it is to be hoped that his splendid example of unselfish devotion to the cause of education will not be allowed to go unheeded.

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SOME MISAPPREHENSIONS AS TO THE
SIMPLIFIED NOMENCLATURE
OF ANATOMY.*

LET it not be interpreted as indifference to the honor of election to an office held by the lamented Joseph Leidy and Harrison Allen if I express even more profound gratification in another action of this Association at its meeting a year ago, viz., the adoption, without dissent, by such of the members as were sufficiently interested to attend, of the 'Report of the Majority† of the Committee on Anatomical Nomenclature' (*Proceedings*, pp. 27-55).

It was then my hope and expectation to lay aside that matter for a year in favor of others already too long deferred. Least of all did I contemplate making it the subject of the present address. The change of plan is due to considerations which may be summarized thus: As investigators our main purpose is to comprehend; as writers and teachers our first duty is to be clear; when, therefore, we have reason to believe that in the minds of our fellows there is obscurity upon a subject of common interest to which we have given particular attention we should avail ourselves of any special opportunity of elucidation, the imperativeness of this obligation being directly proportionate to the personal, professional and official importance of those who seem to need enlightenment.

When, therefore, it is announced that at this meeting the Association will be called upon, in respect to nomenclature, to 'reconsider its acts from the beginning' ('Minority Report,' p. 57); when those who make this announcement are among the original members of the Association and its only surviving past Presidents; when, upon both sides of the water, there have

been published reports, articles, reviews and paragraphs in books* containing, however unintentionally, statements so inadequate, exaggerated, or even inaccurate, as to mislead those not themselves acquainted with the facts; and when, finally, it is probable that the facts are more familiar to me than to any other single individual, it becomes not merely my privilege, but my duty, to share my information with the members of this Association and with others interested who may have lacked the time or opportunity to gain it hitherto.

So numerous are the misapprehensions as to the nature of the simplified nomenclature and the purposes of its advocates that it is impossible to consider them all fully upon the present occasion; some, indeed, will be merely stated in the hope that such

* 1. Verhandlungen der anatomischen Gesellschaft auf der neunten Versammlung, in Basel, April, 1895. *Anat. Anzeiger*; Ergänzungsheft zum X. Band; p. 162.

2. His, W.—Die anatomische Nomenclatur. Nomina anatomica. Verzeichniss der von der Anatomischen Gesellschaft auf ihrer IX. Versammlung in Basel angenommenen Namen. Eingeleitet und im Einverständniss mit dem Redactionsausschuss erläutert. *Archiv für Anatomie und Physiologie*. Anat. Abth., Supplement Band, 1895. O., pp. 180; 27 figs., 2 plates, 1895; [pp. 6-7].

3. Herr Burt Wilder und die Anatomische Nomenclatur. *Anat. Anzeiger*, XII., 446-448, Oct. 30, 1896.

4. Kölliker, A. von.—Handbuch der Gewebelehre des Menschen. Sechste Auflage. Zweiter Band. Nervensystem des Menschen und der Thiere. O., pp. 874, 845 figs. *Leipzig*, 1896; [p. 814].

5. Dwight, Thomas.—Wilder's System der Nomenclatur. *Ergebnisse der Anatomie und Entwicklungsgeschichte*, 1897, pp. 471-479.

6. Baker, Frank.—Review of the foregoing. *SCIENCE*, VII., 715-716, May 28, 1898.

7. Baker, F., and Dwight, T.—Report of the Minority of the Committee on Anatomical Nomenclature. *Proceedings of the tenth annual session of the Association of American Anatomists*, December 28, 1897, pp. 55-57.

8. Reviews of Mills, 'The Nervous System and its Diseases,' in various medical journals; 1898.

* Address of the President at the opening of the eleventh annual session of the Association of American Anatomists, December 28, 1898.

† F. H. Gerrish, Geo. S. Huntington and myself.

statements may carry their own correction. Certain points were presented two years ago.* If, in a few instances, I repeat what I have previously published, precedent for so doing may be found in these words of Huxley:

"When objections are ignored without being refuted or even discussed, I suppose the best way is to emphasize them afresh." *Zool. Soc. Proceedings*, 1883, p. 139.

Misapprehension I.† That the 'Majority Report' embodies the positive convictions of one member and the merely passive acquiescence of the other two.—Such an impression not only might be, but actually has been, produced by the 'Minority Report.' Nothing could be less accurate or just.

The members of this Association need only be reminded that the two other signers of the 'Majority Report' are among the more active of our associates; that they are writers, and are, or have been, practi-

tioners; and that they are teachers of anatomy in long-established medical schools.

But even more significant in this connection is something best known to those who know them best. These men, in a notable degree, combine intellectual independence with liberality; in other words, they are conspicuously free from two qualities shared by the human species with certain other mammals, *viz.*, uncritical imitation, on the one hand, and, on the other, hostility toward what appears to be new merely because they are personally unfamiliar with it.

With regard to the matter in question, as was expressly stated in the 'Majority Report' (p. 31, § 2, 5), "with few exceptions the terms recommended had been adopted by each member individually, and prior to the conference at which joint action was taken."*

Notwithstanding the nature of their convictions, if the larger number of those in attendance at the present session decide to materially modify or even reverse the action of a year ago, the majority of your committee will offer no factious opposition.† They will, however, feel none the less proud of their work and confident of its eventual readoption. Their sentiments may be compared, although somewhat remotely, with those of the surgeon who had devised a new flap for amputation of the thigh. Upon the first trial, just as the operation was triumphantly completed, an overdose of chloroform killed the patient. "Too bad," said the surgeon, "but at any rate he'll go to heaven with the best flap that ever was made."

* For the complete appreciation of the situation it should perhaps be added that the two other signers of the 'Majority Report' were appointed on the Committee respectively by the two signers of the 'Minority Report' while serving as Presidents.

† At the closing session (December 30, 1898) of the eleventh meeting the second Report of the Majority of the Committee was adopted by the Association.

* Neural Terms, International and National, *Journal of Comparative Neurology*, VI., December, 1896, pp. 216-352, including seven tables. Parts VII.-IX. have also been reprinted under the title 'Table of Neural Terms, with Comments and Bibliography,' including also 'Suggestions to American Anatomists.' Copies of the entire paper and also of the 'Tables,' etc., were sent to all members of all committees on nomenclature, here and abroad, and to many other anatomists and neurologists. To them were also sent copies of the 'Table,' etc., and the latter was still more widely distributed to others more or less directly interested in the subject. My reprints of the entire paper are exhausted; of the 'Table,' etc., some copies remain that will be sent upon application. The larger part of the paper is contained in the lecture 'Some Neural Terms,' in 'Biological Lectures' [at the Marine Biological Laboratory] for 1896-7. The 'Errors and Omissions' detected in my Lists of Neural Terms have been corrected in the *Journal of Comparative Neurology*, VIII., pp. li-iii, July, 1898; a leaflet reprint has been inserted in copies of 'Neural Terms' and of 'Table of Neural Terms' distributed since March 30, 1898, and will be sent upon request to those who received copies prior to that date.

† The succeeding misapprehensions will be designated simply by Roman numerals.

II. *That any action of the Association with respect to the use of terms has binding force.*—From certain expressions it might be inferred that the adoption of a report on nomenclature was tantamount to the enactment of rules or by-laws, conformity to which constitutes an indispensable condition of the maintenance of membership. On the contrary, the recommendation and acceptance of certain terms merely entitles them to particularly respectful consideration and throws upon those who prefer others the burden of proof that those others are superior. As an illustration of the impunity with which somewhat stringent injunctions may be disregarded may be mentioned the following: In the *Anatomischer Anzeiger* (March 3, 1897, pp. 323–329), in a paper by Dr. Edward Flatau, 'Beitrag zur technischen Bearbeitung des Centralnervensystems,' prepared in the Anatomic Institute at Berlin, the Director of which is Professor Waldeyer, a member of the B. N. A. Commission and of the Gesellschaft that recommended *Dura mater encephali* and *Pia mater encephali*, the mononyms *dura* and *pia* occur two and four times respectively, and the authorized polyonyms are conspicuous by their absence.

III. *That action of the majority of a committee should be delayed indefinitely by the absence or unpreparedness of the minority after due notice is given.*

IV. *That the condemnatory phrases of the 'Minority Report' can, in any considerable degree, be justly applied to the actual contents of the 'Majority Report.'*

V. *That the non-adoption of a term, whether from the German list or my own, constitutes a declaration against it.*—It signifies merely a suspension of judgment and a postponement of action.

VI. *That differences of usage or recommendation between American and foreign anatomists or organizations should be removed in all cases by the abandonment of our position.*

VII. *That the efforts of this Association for the simplification of nomenclature should be paralyzed by the disapprobation of foreign anatomists whose unfamiliarity with what is done in America is to be explained only by an indifference thereto.*—Among numerous instances of this indifference I select one with which my own connection is so remote as to eliminate the element of personal irritation. At the meeting of this Association in December, 1895, there was presented an elaborate 'Report on the Collection and Preservation of Anatomical Material.' It was printed in our *Proceedings* (15–38) and in *SCIENCE*, III., January 17, 1896; was mentioned in several journals and listed in the 'Literatur' in the *Anatomischer Anzeiger*. Yet in September, 1898, practically an entire number of that periodical, twenty-five pages, was occupied by an article on that subject purporting to tabulate and discuss the methods employed in all parts of the world. The whole United States is credited with an article by Mall (*Anzeiger*, 1896, 769–775) and (in a footnote) a 'Note' by Keiller in the *Texas Medical Journal*, 1891–2, VII., p. 425.

VIII. *That terms consisting of a single word each constitute even the majority of the names preferred by me or adopted by this Association a year ago.*—Whatever their abstract preferences, the members of the Committee realize the impossibility of framing such a nomenclature. Two years ago ('Neural Terms,' § 153 *et seq.*) I showed by statistics the baselessness of the misapprehension and characterized it as a 'terminologic phantasm erected by the Germans between themselves and the American Committees.'

More recently, however, the same notion has reappeared in several reviews of a textbook of nervous diseases, commonly with approval, expressed or implied, of the supposed condition. The impression was probably gained from the fact that the author of the book, like myself, prefers single-word

names for as many as possible of the parts most frequently mentioned. Nevertheless, the misapprehension on this point ought to be corrected. The facts are:

First, out of about 540 neural terms in the B. N. A. at least 40, about one-fourteenth, are mononyms.

Secondly, in the 'Majority Report', in Tables C and D, are enumerated 274 terms differing more or less from those adopted by the Gesellschaft; the mononyms number only 103.

IX. *That eminence as an anatomist necessarily implies either the capacity or the disposition to deal wisely with questions of nomenclature.*—Upon this point I quote from 'Concluding Remarks' in 'Neural Terms,' p. 329:

Caution in Publishing New Terms.—It is true that words needlessly introduced into anatomy have no such embarrassing permanency as is conventionally assigned to synonyms in systematic zoölogy. Nevertheless, for a time at least, they encumber current publications and dictionaries. Hence, however necessary and legitimate they may seem to the framer, neither a new term, nor an old one in a new sense, should be actually published without prolonged consideration, and consultation with at least four individuals representing as many categories of possible critics: (a) an investigator of the same general subject; (b) an experienced teacher; (c) an earnest student; (d) a philologic expert whose admiration for the past has not blinded him to the needs of the present and the future.

Method of Introduction of New Terms.—As urgently recommended by the A. A. A. S. Committee on Biological Nomenclature, whenever a technical word is used for the first time the author should give in a special note: (a) the Latin form; (b) the etymology; (c) the proper adopted form or paronym for his own language, with the adjective, etc., when applicable; (d) as concise and precise a definition as possible.

X. *That among the terms included in the 'Majority Report' any considerable number have been specifically condemned by the Anatomische Gesellschaft or its authorized representatives.*

XI. *That the grounds of such objections as have been offered are really sound and sufficient.*

XII. *That the condemnation of a term by an anatomic authority disproves either its intrinsic fitness or its promise of vitality.*—On this point there need be adduced only the cases of *radius* and *ulna*, which Robert Hunter denounced as 'ridiculous.'

XIII. *That the anatomy of the future is to be based upon the structure and erect attitude of the human body.*—The anatomists of the future will be zoötomists first and anthropotomists afterward.

XIV. *That every anatomic term should be an absolute idionym, i. e., perfectly explicit in itself.*—Since this requirement is implied in the objections to *aula*, etc., by Kölliker, and to *medipedunculus* by His,* there may be properly adduced from the B. N. A. the following terms, whose explicitness is conditioned upon either the context or the actual addition of the words here set in brackets: *clivus* [*occipitalis*], and [*sphenoidalis*]; *processus coronoideus* [*ulnæ*] and [*mandibulæ*]; *processus styloideus* [*radii*], [*ulnæ*], and [*ossis temporalis*]. Unless, indeed, it be granted that a certain degree of explicitness is afforded by the context, every one of the thousands of names of the parts of the human body should be increased by the phrase *corporis humani*.

XV. *That the occasional employment, by a member of an Association, or even by a member of its Committee on Nomenclature, of terms other than those adopted by them is, in itself, evidence of deliberate intention.*—For example, after using *conarium* for fifteen years in place of 'pineal body,' etc., now that the arguments of Spitzka and H. F. Osborn have converted me to *epiphysis*, *conarium* occasionally gets itself spoken. Indeed, it is easy for me to understand that an unintended but familiar word may be written, re-written, and even overlooked in the proof. The frequency of such lapses could be shown, if necessary, by letters from numer-

*As stated and briefly discussed in 'Neural Terms,' pp. 282-289.

ous correspondents in reply to the query, free from all critical or proselytic tenor, as to whether a given term was used intentionally or by inadvertence.

XVI. *That there is 'imminent danger of the formation of a peculiar anatomic vocabulary in America such as seriously to impede scientific intercourse with other countries.'*—The unsubstantiality of the grounds of this misapprehension may be recognized in the impartial discussion by the brothers Herrick a year ago.* They conclude that there is no reason for serious alarm on this score.

XVII. *That the fundamental principles and characteristic features of the simplified nomenclature can be attributed to any individual in such degree as to warrant calling it by his name.*—In correcting this misapprehension no false modesty shall lead me to belittle what I have done. On the contrary, to the 'Summary of my terminologic progress,' already published in 'Neural Terms,' etc. (pp. 227-237), there shall be added here two items overlooked when that was printed:

1. That the defects of encephalic terminology had been recognized by me as early as 1873 may be seen from the following paragraph in a popular lecture on 'The brain and the present scientific aspects of phrenology,' delivered January 21st, before the 'American Institute,' and reported in the *New York Tribune* of January 22d and in the '*Tribune Extra*,' No. 3:

"As if these natural hindrances were not enough, the old anatomists fenced in the parts of the brain with the most fanciful and prodigious titles. *Cerebrum* is well enough; the *cerebellum*, being only one-eighth as large, has a longer name, while *medulla oblongata*, *hippocampus minor*, *tubercula quadrigemina*, *processus e cerebello ad testes*, and *iter e tertio ad ventriculum quartum* represent such insignificant parts of the brain as to suggest a suspicion that the nomenclature was established upon no other principle than that of inverse ratio between the size of an organ and the length of its title. At any rate, these fearful names

*Inquiries regarding tendencies current in neurological literature; *Jour. Comp. Neurology*, VII., 162-168, December, 1897.

are stumbling-blocks to the student and an almost perfect hindrance to popular knowledge of the brain; no doubt this pleases the ghosts of the old anatomical fathers, and is equally agreeable to many of the present day, both in and out of the profession, with whom Latin is a synonym for learning, and ponderosity of words for profundity of wisdom."

2. My actual efforts toward the simplification of the nomenclature of the brain commenced in 1880, in the preparation of a paper read before the American Association for the Advancement of Science on the 28th of August. The paper was never written out in full, and apparently no abstract was furnished for publication in the *Proceedings*. Somewhat inadequate and erroneous reports were printed in the *Boston Daily Advertiser* of August 30th, and in the *New York Medical Record* of September 18th. But here is a duplicate of the abstract furnished in advance to the Secretary of the Association, and I venture to read it as a contribution to the history of the subject now before us:

"PARTIAL REVISION OF THE NOMENCLATURE OF THE BRAIN.

"A. *Introductory*: The progress of anatomy is impeded by the defects of nomenclature. These defects have been admitted by several anatomists, and a few have endeavored to remedy them. As stated by Pye-Smith, 'the nomenclature of the brain stands more in need of revision than that of any other part.'

"B. *Nature of the Defects*: (1) General. In common with that of the rest of the body, the nomenclature of the brain lacks precision as to the position and direction of parts. (2) In particular the number of synonyms is very large. Most writers employ some names which are vernacular or merely descriptive. Most technical names are compound; many of the single ones are inconveniently long, and some of them are indecent.

"C. *Special Obstacles to a Reform*: (1) The difficulty of ascertaining the priority of terms. (2) The tendency of each nation to adopt purely vernacular terms which have been proposed or incidentally employed by eminent anatomists of that nation.

"D. *Principles Forming the Basis of this Revision*: (1) Technical terms are the tools of thought, and the best workman uses the best tools. (2) Terms of classical origin are to be preferred. (3) Priority of

employment is to be regarded, but should not overbear all other considerations. (4) Of two terms equally acceptable in other respects, to select the shorter. (5) Preference for names of general application over those which have an exclusive application to man or the other primates. (6) To convert some compound terms into simple ones, either by dropping unessential words or by the substitution of prefixes for adjectives. (7) For terms of position, to discard all which refer to the horizon or to the natural attitude of man, and to adopt those which refer to the longitudinal axis of the vertebrate body. (8) For terms of relative position and direction, to employ those used for position with the termination *ad*.

"E. *The Paper Will Indicate*: (1) The terms proposed and their abbreviations. (2) The principal synonyms. (3) The originators of the terms and synonyms and the dates of their first employment, so far as ascertained. (4) The terms which should be wholly discarded. (5) The new terms for new parts, the new terms for parts already known, the new forms of old terms. (6) The subordination of parts to wholes by differences in the kinds of type."

There were present Harrison Allen, Simon H. Gage, Charles S. Minot and probably other members of this Association; the survivors will recall that on cloth sheets were written in parallel columns certain names in common use, together with those which were proposed to replace them. Amongst these were *pons* for 'pons Varolii'; *insula* for 'insula Reillii'; *thalamus* for 'thalamus opticus'; *callosum* and *striatum* for 'corpus callosum' and 'corpus striatum'; *præcommisura* for 'commissura anterior'; *myelon* for 'medulla spinalis,' and *cornu dorsale*, for 'cornu posterius.' This paper constituted the proton (the *primordium*, or 'Anlage,' if you prefer) of my own subsequent contributions, and likewise, so far as I knew at the time, of the simplified nomenclature in America.

Proud as I am of these early propositions, and glad as I should be if they and their subsequent elaborations had been at once unprecedented and sufficient, nevertheless truth, justice and the peculiar conditions now confronting us alike impel me upon this occasion to insist even more distinctly

than hitherto upon the extent to which the ideas and even the specific terms had been anticipated by four other anatomists in this country and in England.

Already in the spring of 1880, although quite unknown to me, there had been published a paper by E. C. Spitzka, 'The Central Tubular Gray' (*Journal of Nervous and Mental Disease*, April, 1880), containing (p. 75, note) the following pregnant paragraph:

"It would add much to the clearness of our terminology, in my opinion, if the adjectives anterior and posterior were to be discarded. Physiologists and anatomists are so often forced to deal with the nerve axes of lower animals, in whom what is with man the anterior root becomes inferior, and what is in the former posterior becomes superior, that they have either been confused themselves or have written confusedly, or finally have, to avoid all misunderstanding, utilized the terms applicable to man alone also for quadrupeds. The nervous axis, however, occupies one definite position, which should determine the topographical designations. What in man is the anterior, and in quadrupeds the inferior, root or cornu is always *ventral*; while what in the former is posterior, and the latter superior, is always *dorsal*. The present treatise is not the proper place for renovating nomenclature, but I have thought it well to call attention to the matter in passing, and in anticipation of a work on comparative neural morphology which I have in preparation."

The concluding words are italicized by me in order that there may be the more fully appreciated the generosity, indeed self-abnegation, exhibited in Dr. Spitzka's commentary* upon my longer paper† of the following year:

"It is with mingled pleasure and profit that I have read the very suggestive paper on cerebral nomenclature contributed to your last issues by Professor Wilder. Some of the suggestions which he has made have been latent in my own mind for years,

* Letter on nomenclature, *SCIENCE*, April 9, 1881. Also in *Jour. Nerv. and Mental Dis.*, July, 1881, 661-662.

† A partial revision of anatomical nomenclature, with especial reference to that of the brain, *SCIENCE*, II., 1881, pp. 122-126, 133-138, March. Also *Jour. Nerv. and Mental Dis.*, July, 1881, 652-661.

but I have lacked the courage [time?] to bring them before my colleagues. Now that he has broken ground, those who prefer a rational nomenclature to one which, like the present reigning one, is based upon erroneous principles, or rather on no principles at all, will be rejoiced at the precedent thus set for innovations. * * * He who has himself been compelled to labor under the curse of the old system, the *beneath, below, under, in front of, inside, external, between, etc.*, will look upon the simple *ventral, dorsal, lateral, mesal, cephalic, proximal, caudal, distal, etc.* as so many boons. I have no hesitation in saying that the labor of the anatomical student will be diminished fully one-half when this nomenclature shall have been definitely adopted. * * * In proceeding to comment on some of the terms proposed by Professor Wilder, I wish it to be distinctly understood that I do so merely tentatively and to promote discussion; in so doing I feel certain that I am carrying out that writer's wishes. It is but just to state that the majority of the terms cannot be discussed; they are perfection and simplicity combined."

Had Dr. Spitzka completed his proposed work he would doubtless have called attention to our three British predecessors, John Barclay, Richard Owen and P. H. Pye-Smith.

The first, as long ago as 1803, in 'A New Anatomical Nomenclature,' proposed the unambiguous descriptive terms, *dorsal, lateral, proximal*, with their adverbial forms, *dorsad, laterad* and *proximad*, and thus laid the foundation for an intrinsic toponymy.

In 1846 Owen published ('Report on the Vertebrate Skeleton,' p. 171) what I have elsewhere ('Neural Terms,' § 51) called the 'immortal paragraph,' wherein the various phrases for the spinal portion of the central nervous system were replaced by the single word, *myelon*. Twenty years later he uttered ('Anatomy of Vertebrates,' I., 294) a declaration which some of us are disposed to regard as an inspired prophecy:

"Whoever will carry out the application of neat substantive names to the homologous parts of the encephalon will perform a good work in true anatomy." In the third volume of the same work (1868, p. 136) is a list of the cerebral fissures designated, in most cases, by adjectives of a single word each, *e. g., subfrontal*.

The paper of Pye-Smith (fortunately still spared to us) was entitled 'Suggestions on Some Points of Anatomical Nomenclature,' and appeared in 1877 (*Journal of Anatomy and Physiology*, XII., 154-175, October, 1877). After enunciating certain sound general principles, he declared that 'the nomenclature of the brain stands more in need of revision than that of any other part,' and made several specific suggestions some of which have been adopted by the three American Associations and the Anatomische Gesellschaft:

"The term optic thalamus is a misleading and cumbersome abbreviation of the proper name *thalamus nervorum opticorum*, and the name *thalamus*, without qualification, is at once distinctive, convenient, and free from a false suggestion as to the function of the part. * * * Of all the synonyms of the *Hippocampus minor* (*Ergot of Morand, eminentia unciformis, colliculus, unguis, calcar avis*) the last is the most distinctive, and brings it at once into relation with the calcarine fissure. The *Hippocampus major* may then drop the adjective, as well as its synonym of *cornu ammonis*. The pineal and pituitary bodies are more conveniently called *conarium* and *hypophysis*. * * * The word *Pons* (Varolii) might well be restricted to the great transverse commissure of the cerebellum. * * * *Insula* is a far more distinctive name than any proposed to replace it." Pye-Smith also prefers *vagus* to 'pneumogastricus.' (p. 162).

Those who have done me the honor to read any one of my longer papers on this subject will recall my repeated acknowledgments of indebtedness to these three English anatomists. Not to mention earlier publications, in 1889, in the article 'Anatomical Terminology' ('Reference Handbook of the Medical Sciences,' VIII., 520-522), Professor Gage and I collected from all sources accessible to us 'Aphorisms respecting Nomenclature;' the most prolific sources were the three just named. At the third meeting of this Association, in Boston, December, 1890, I read a paper the title of which was 'Owen's Nomenclature of the Brain,' and which included this paragraph:

"In none of the above-designated publications or in those of other anatomists does it now seem to the

writer that there has been adequate recognition of the terminological precepts and examples that occur in the works of Professor Richard Owen, and the writer takes this opportunity to express his constantly increasing sense of obligation in this regard; had space permitted he would gladly have increased the number and length of the selections from Professor Owen's writings which are embraced among the 'Aphorisms respecting Nomenclature' on pp. 520-522 of the article 'Anatomical Terminology.' "

In this connection may appropriately be mentioned two later but highly significant British contributions toward a simplified and international system of nomenclature.

1. The Latin names for the encephalic segments.—In the seventh edition of Quain's 'Anatomy', edited by William Sharpey, Allen Thompson and John Cleland, in Vol. II., dated 1867, the five 'fundamental parts' (corresponding to what I have called 'definitive segments') are named *prosencephalon*, *diencephalon*, *mesencephalon*, *epencephalon*, and *metencephalon*; and in a foot-note these terms are declared to be "adopted as applicable to the principal secondary divisions of the primordial medullary tube, and as corresponding to the commonly received names of the German embryologists, viz., *Vorderhirn*, *Zwischenhirn*, *Mittelhirn*, *Hinterhirn*, and *Nachhirn*; or their less-used English translations, viz., *forebrain*, *interbrain*, *midbrain*, *hindbrain*, and *afterbrain*."

Notwithstanding several public requests for information as to the source of the Latin segmental names, the historic facts recorded in the above extract were ascertained by me only within the past week; I prefer to believe that they were unknown to the Nomenclature Commission and to the Anatomische Gesellschaft at the time of the selection and adoption of the Latin names for the encephalic segments as given in the B. N. A. Even, then, however, since the same Latin terms were repeated in the subsequent editions of Quain (1877-1882), I am compelled to regard the transference of *metencephalon* from the ultimate segment to

the penultimate, and its replacement by *myelencephalon*, as constituting a violation of scientific ethics that merits the severest reprobation. *

2. Mononymic designations of the encephalic cavities.—In August, 1882, wholly unaware of my prior suggestion to the same effect (SCIENCE, March, 1881), the late T. Jeffery Parker, professor in Otago University, New Zealand, proposed compounds of the Greek *zotlia*, with the prepositions, etc., already employed in the segmental names; e. g., *mesocæle*, *prosocæle*, etc. Our mutual gratification and encouragement at the approximate coincidence led to a cordial correspondence that continued until his death. Besides the publications enumerated in the Bibliography of 'Neural Terms,' Parker used celian compounds in two papers on the *Apteryx* (1890 and 1892) and in the 'Text-book of Zoology' by himself and Professor Haswell (1897).

XVIII. *That, even in its earliest and crudest form, the 'system' sometimes called by my name could fairly be characterized as 'generally repulsive' and as having 'not the slightest chance of general adoption.'* †—On this point it is sufficient to introduce the following letter ‡ from Oliver Wendell Holmes, whose point of view was at once that of the literary critic and the experienced teacher of anatomy in a medical school:

"BOSTON, May 3, 1881.

"DEAR DR. WILDER: I have read carefully your paper on Nomenclature. I entirely approve of it as an attempt, an attempt which I hope will be partially successful, for no such sweeping change is, I think, ever adopted as a whole. But I am struck with the

* The intrinsic merits of various segmental names have been discussed by me in 'Neural Terms,' etc., 326-328, and in the *Proceedings of this Association* for the ninth session, May, 1897, 28-29.

† These phrases occur in the 'Minority Report.'

‡ As a whole or in part this notable document has been printed previously in SCIENCE, May 28, 1881; in 'The Brain of the Cat,' *Amer. Philos. Soc., Proceedings*, XIX., p. 530, 1881; 'Anatomical Technology,' 1882, p. 11; 'Neural Terms', p. 237.

reasonableness of the system of changes which you propose, and the fitness of many of the special terms you have suggested.

"The last thing an old teacher wants is, as you know full well, a new set of terms for a familiar set of objects. It is hard instructing ancient canine individuals in new devices. It is hard teaching old professors new tricks. So my approbation of your attempt is a *sic vos non vobis* case so far as I am concerned.

"What you have to do is to keep agitating the subject; to go on training your students to the new terms, some of which you or others will doubtless see reasons for changing; to improve as far as possible, fill up blanks, perhaps get up a small manual in which the new terms shall be practically applied, and have faith that sooner or later the best part of your innovations will find their way into scientific use. The plan is an excellent one; it is a new garment which will fit Science well, if that capricious and fantastic and old-fashioned dressing lady can only be induced to try it on.

"Always very truly yours,

"OLIVER WENDELL HOLMES."

XIX. *That, at the present stage of the subject, it is possible for any individual, however impartial and well informed, to wholly avert the possibility of misapprehension or even injustice, in attempting to indicate the attitude of living anatomists toward the simplified nomenclature.*—My impartiality may perhaps be challenged, but I am at least familiar with current literature in this respect; moreover, since 1880 I have preserved all letters in which the matter is considered. Probably no one agrees with me absolutely and in every respect. On the other hand, even some frankly avowed opponents now assent to what they would have regarded as quite heretical a few years ago.*

XX. *That whatever misapprehension may exist in this country or abroad as to the degree in which the terms or principles advocated by me are indorsed by others can be justly ascribed to either unfounded declarations or intimations on my part, or to the omission of definite efforts to avert or*

*In the verbal presentation of a paper at this meeting Professor Dwight designated the costiferous vertebrae as *thoracic* rather than *dorsal*, with a consistency both gratifying and encouraging.

remove such misapprehension.—The enumeration of the conditions that led to the preparation of 'Neural Terms' included (p. 217) the following sentence: "I particularly desire to free the committees, their individual members, and the associations which they represent, from responsibilities not yet assumed by them." More or less explicit and emphatic affirmations to the same effect occur on pp. 273, 295, 299 and 301.*

XXI. *That 'most scholars are repelled by' my 'fantastic terms and defects of literary form.'*—This assertion occurs in the 'review' (No. 6), and presumably refers to the 'system' in its present or recent state. The position taken is apparently impregnable, since for every one who has declared his adhesion there might be named a score who have said nothing about it. Seriously, however, it is not easy to discuss such a proposition without adducing evidence that might fairly be challenged by one side or the other. At any rate, in the present connection I shall omit my more or less intimate friends and correspondents, living and dead; Harrison Allen, W. R. Birdsall, Oliver Wendell Holmes, Joseph Leidy, and E. C. Seguin; William Browning, Joseph Collins, Elliott Coues, H. H. Donaldson, F. H. Gerrish,

*At the meeting of the American Medical Association in Philadelphia, June, 1897, the Section on Neurology and Medical Jurisprudence adopted the following resolution, recommended by the Committee on the Address of the Chairman, W. J. Herdman:

"Resolved, That the Section of Neurology and Medical Jurisprudence endorse the neural terms adopted by the American Neurological Association, the Association of American Anatomists, and the American Association for the Advancement of Science, and so far as practical recommend their use in the work of the section.

C. K. MILLS,
C. H. HUGHES,
HAROLD N. MOYER."

Since the action above recorded was taken in June, 1897, it does not, of course, apply to the subsequent adoptions by this Association at the tenth and eleventh sessions; Dec., 1897, and Dec., 1898.

George M. Gould, the brothers Herrick, G. S. Huntington, C. K. Mills, W. J. Herdman, H. F. Osborn, C. E. Riggs, D. K. Shute, Sorenson, Spitzka, O. S. Strong, W. G. Tight, C. H. Turner, A. F. Witmer and R. Ramsay Wright; also past or present pupils or colleagues, T. E. Clark, P. A. Fish, S. H. Gage, Mrs. Gage, G. S. Hopkins, O. D. Humphrey, A. T. Kerr, B. F. Kingsbury, W. C. Krauss, T. B. Stowell and B. B. Stroud. I have now, I think, eliminated all whose more or less complete adoption or approval of my 'system' might be ascribed in some degree to personal considerations.*

There has lately been afforded me, however, the desired opportunity of collating the impressions of a somewhat homogeneous group of scholars, quite unlikely to have been influenced by a disinclination to antagonize my views. Through the courtesy of

*Curiously enough, in the single instance of the apparent operation of personal influence, the individual was of German descent and we had met but once. Prior to our meeting in December, 1895, I prepared a typewritten list of the neural terms that had been adopted earlier in the year by the Anatomische Gesellschaft, and in parallel columns added those preferred by me. Copies of this list were sent to members of the Association as a basis for the anticipated discussion. In January the late Dr. Carl Heitzmann, in acknowledging his copy, accounted at the same time for his absence from the meeting: "My intention was to urge the acceptance of the nomenclature adopted by the German Anatomical Society, deficient as it is, simply to obtain uniformity. * * * Personally I cannot vote against you; hence I rather abstain from coming to the meetings till this matter will be settled."

My response was as follows: "Your letter affects me deeply, and were my efforts toward the improvement of anatomical nomenclature for my own sake or for the present at all it would go far to deter me from further persistence. But I never lose sight of the fact that we of to-day, and even the honored workers of the past, are few and insignificant as compared with our successors, and I do not mean to be reproached by them for failing to do what I can. Do not refrain from writing, publishing or voting against me according to your convictions. It will come out right in the end."

the author of a recent American text-book on 'The Nervous System and its Diseases,' in which the simplified nomenclature is fully and expressly employed, I have been enabled to read all the reviews of it that have thus far appeared. For the sake of homogeneity I have excluded two non-medical journals, the *Revue Neurologique*, which says nothing on the subject of nomenclature, and the *Journal of Comparative Neurology*, which, upon the whole, is favorable. This leaves thirty reviews of a book intended for students; reviews written by practitioners, some of them well-known experts and also teachers of neurology. As such, upon general principles, any modification of the current terminology must be more or less unwelcome to them.

Upon the basis of their attitude toward the simplified nomenclature the reviews fall naturally into four groups, viz.: A, those that ignore the subject (8, about 27 per cent.); B, those that merely mention it (6, 20 per cent.); C, those that condemn the introduction of the simplified terms more or less decidedly (6, 20 per cent.); D, those that commend it (10, 33 per cent.). Without going so far as to reverse the Scriptural saying and claim that 'he who is not against us is with us,' we may infer that the fourteen reviewers in groups A and B were at least not 'repelled' by the simplified terms; on the contrary, many of them call attention to the clearness and accuracy of the anatomic and embryologic sections of the book where, of course, the terms are most conspicuous.

In category C I have included one that might, without real unfairness, have been left in category B; in the *Colorado Medical Journal*, after characterizing the anatomic portion of the work as 'especially excellent,' Dr. Eskridge simply expresses the 'fear that the new nomenclature will not meet with general favor.'

The six antagonistic reviews are con-

tained in the *Pacific Record of Medicine and Surgery*, the *London Lancet*, the *Colorado Medical Journal*, the *American Journal of Insanity*, the *New York Medical Record* and the *Journal of Nervous and Mental Disease*. I quote from the last two as highly influential and representative. The *Record* says:

"There is to be found an ample, clear and thoroughly scientific treatment of the anatomy of the nervous system. * * * We are not in thorough sympathy with nomenclatural cataclysms, and feel that frequently the old and familiar is clothed in new terms for the sake of lending an air of novelty and apparent gloss of 'science.' Still in the biological sciences nomenclature forms one of the most important landmarks of progress, especially when by it new and wider conceptions are gained. We believe, however, that in the adoption of the Wilder terminology the author has departed from a healthy historical conservatism, but this is, perhaps, an academic matter after all."

The foregoing contains so many qualifications as to leave its purport somewhat in doubt; indeed, one may imagine its writer, as he finished it, exclaiming, with the Congressman, 'Where am I at?'

The remarks of Dr. B. Sachs in the *Journal of Nervous and Mental Disease* are more explicit, and I should be glad to reproduce them in full; on the present occasion extracts must suffice:

"It is to be feared that the student will not be grateful for the introduction of the new cerebral terminology of Wilder and Gage. While recognizing the full merits of the new nomenclature and appreciating the benefits conferred upon the comparative anatomist and the comparative embryologist, the truth is, the student of neurology does not need it. * * * It has been suggested that children should begin the study of brain anatomy. The plan is a good one with reference to this nomenclature; the only way to acquire it is to acquire it early in life, when the cortical cells are ready for the reception of any and all auditory impressions. We have no doubt that in the course of time some of these names will be adopted by general consent; but it will be well along in the next century before the system, as a whole, will come into use."

Upon the whole I find myself less depressed by the objections of Dr. Sachs than

encouraged by his almost startling forecast. He is young enough for me to venture the prediction that 'well along in the next century' he will be surrounded by colleagues and pupils who, according to my plan,* commenced the practical study of the brain in the primary school, and who, by the aid of the simplified nomenclature, learned twice as rapidly as ourselves.

Among the ten favorable reviews the most elaborate is in the *Journal of the American Medical Association* (August 20, 1898). That in the *New York Medical Journal* (May 21, 1898) concludes thus:

"We are very glad that the author has had the courage to introduce these terms, believing, as we do, in their correctness and in the need of their becoming familiar."

I refrain from reading the other reviews† in Group D, mainly because the expressions therein complimentary to myself are embarrassingly numerous and emphatic. In view of this evidence those who contend that 'most scholars are repelled by my fantastic terms and defects of literary form' would seem called upon to either withdraw that claim as a misapprehension or to modify materially the commonly accepted definition of medical and scientific scholarship.

XXII. That 'barbarisms' constitute an objectionable feature of my 'system.'—Upon the supposition that by barbarisms are here meant hybrid words, this point was somewhat fully discussed in 'Neural Terms,' p. 290. Since the criticism was offered by the

* The desirability and feasibility of the acquisition of some real and accurate knowledge of the brain by precollegiate scholars. *Amer. Soc. Naturalists Records*, p. 31, 1896; *SCIENCE*, December 17, 1897.

† The *St. Louis Medical and Surgical Journal* (April, 1898); (Portland, Oregon) *Medical Sentinel* (April, 1898); (Detroit) *Medical Age* (April 11, 1898); *Canada Lancet* (May, 1898); *Richmond (Va.) Journal of Practice* (May, 1898); *Buffalo Medical Journal* (June, 1898); *University (of Pa.) Medical Magazine* (September, 1898); *North Carolina Medical Journal* (September, 1898).

chairman of the Nomenclatur Commission, Professor Kölliker, it might naturally be inferred that the list of terms adopted by that body is free from hybrid words. Yet not only does the B. N. A. contain several such, but certain of them are less euphonious than most of those for which I am responsible. Comparison is invited between the Græco-Latin combinations in the two following groups, the first from my list, the second from the B. N. A.; in each case the Greek element is printed in italics: *Metatela*, *diatela*, *paratela*, *metaplexus*, *diaplexus*, *paraplexus*, *ectocinerea*, *entocinerea*, *hemice-rebrum*, *hemiseptum*; *epidurale*, *mesovaricus*, *parumbilicales*, *parolfactorius*, *suprachorioi-dea*,* *pterygopalatinus*, *pterygomandibularis*, *phrenicocostalis*, *sphenopalatinum*, *spheno-oc-cipitalis*, *occipitomastoidea*, *squamosomas-toidea*.

XXIII. *That progress toward the right solution of the questions involved is really facilitated by general denunciations of a given system or its advocates.*—The attitude of some may be likened to that of the child in the lines:

"I do not love thee, Dr. Fell,
The reason why I cannot tell,
But this alone I know full well,
I do not love thee, Dr. Fell."

History will record whether such conservatives shall rank with heroic defenders of law and order, or be rated among the Canutes of science, their utterances, in respect to nomenclature, remembered mainly as 'things one would rather have left unsaid.'

History will likewise record whether some others, including, of course, the framers of the 'Majority Report,' shall be metaphorically 'hanged, drawn and quartered'

* In Table IV., p. 290 of 'Neural Terms' (likewise in Biological Lectures, p. 158) *suprachorioidea* was printed without the first (and, as it seems to me, superfluous) *i*; also, most regrettably, there was included in the list *perichorioideale*, a wholly Greek combination.

as rebels, or, notwithstanding errors of judgment, credited with leaving the pathway of future students of anatomy smoother than they found it themselves.

XXIV. *That the English-speaking anatomists who have been laboring long for the simplification of nomenclature are called upon to submit indefinitely to animadversions based upon inertia, lack of information, misapprehension, or undue deference to the adverse pronunciamentos of scientific potentates abroad.*—Speaking for myself alone, the spirit in which I prefer to meet hostile criticism is fairly exemplified in my reply (*N. Y. Medical Record*, Oct. 2, 1886, 389-390) to an article in a leading medical journal containing an egregious and inexcusable misstatement that might readily have led uninformed readers to question the soundness of all my proposals. That article, however, although upon the editorial page, was evidently prepared in haste. But such extenuation will scarcely be urged in the case of the publication numbered 6 in the list in the note on p. 566. This is a review of an article (no. 5), and to avoid confusion I shall speak of the 'article' and its 'author,' of the 'review' and the 'reviewer.'

The review contains this passage:

"Some of the peculiarities of the Wilder system are then briefly discussed [in the article], attention being called to its disregard of the ordinary principles of language formation as exemplified by Ist. The mutilation of words as by using * * * *hippocamp** for *hippocampus major*."

* In the original this is 'chippocamp'. The reviewer promptly assured me that the mistake was the printer's and that it would be 'corrected wherever possible'. I assume that the copies of SCIENCE sent by him to others were emended like that received by me. But, so far as I am aware, no public correction has been made. Under some circumstances this might be regarded as superfluous. But it must be borne in mind that unjustifiable verbifaction constituted the very substance of the indictment; hence the situation was as if John Doe accused Richard Roe publicly of

It may be doubted whether scientific literature can furnish a single sentence of equal length containing so many erroneous statements and implications. For clear discrimination the several points shall be put in the form of questions:

1. In the article purporting to be the source of the criticism quoted is there mentioned either the word *hippocamp* or any other word representing a comparable etymologic category?

In that article, beyond the reproduction of reports including the words *hippocampus* and *hippocampus major*, the single reference is as follows (translated):

"Wilder holds that there is no longer ground for retaining *avis* with *calcar*, a term which is to be used in place of *hippocampus minor*. If this be granted, then naturally the *major* of *hippocampus major* can be dropped. The writer approves of these changes."

2. Is the reviewer himself on record as preferring the apparently alternative term, 'hippocampus major,' to *hippocampus*?

The reviewer, as a member of our Committee on Anatomical Nomenclature, signed the first report, in 1889, which recommended the replacement of 'hippocampus major' by *hippocampus*. Since this change was also adopted in 1895 by the Anatomische Gesellschaft, I have not supposed that its abandonment was embraced within the proposition of the 'Minority Report' that the Association should 'reconsider its acts from the beginning.'

3. Has the word *hippocamp* ever been used or proposed by me in any other status than passing counterfeit money; as if the nature of one of Roe's occupations at the time rendered it particularly desirable that his character be unimpeached; as if part of the evidence against him were a spurious coin that had been dropped into his pocket accidentally by an employee of Doe himself; and, finally, as if Doe held adequate reparation to be made by confining the admission of the mistake to the officers of the law and his personal friends. Nevertheless, in order that the issues before us may be kept free of all points upon which there may be room for diversity of opinion, this mischance shall be hereafter ignored.

that of a national, English form (Anglo-paronym) of the international, Latin *hippocampus*?

The negative answer to this may be found in various publications during the last fifteen years. Among the fuller and more accessible presentations are these passages from 'Neural Terms' (pp. 231-232, 226):

"Each anatomist prefers to employ terms belonging to his own language; at the same time he prefers that others should employ Latin terms with which he is already familiar. *Sea horse*, *Cheval marin* and *Seepferd* are synonyms (in the broader sense, §42), but to either an Englishman, a Frenchman or a German, two of them are foreign words and unacceptable. *Hippocampus* is distinctly a Latin word, and the frequent occurrence of such imparts a pedantic character to either discourse or written page. *Hippocamp*, *hippocampe*, *hippocampo*, and *Hippokamp* are as distinctly national forms of the common international antecedent (not to invoke the original Greek *ἵπποκαμπος*), and are readily recognized by all, while yet conforming to the 'genius' of each language."

4. Does the reduction of *hippocampus* to *hippocamp* represent a group of cases so numerous in even my complete list of neural terms as to constitute a prominent feature of what is called my 'system'?

The list embraces about 440 terms; besides *hippocamp* there are just two cases in which I have been apparently the first to Anglicize Latin words by dropping the last syllable, the inflected ending; viz., *myelon*, *myel*, and *encephalon*, *encephal* (and its compounds).

5. If, finally, every one of the 440 Latin terms happened to consist of a single word ending in either *a*, *ma*, *us*, *on*, *is*, *um*, or *ium*, and if I had proposed that English-speaking anatomists should customarily omit those syllables, would that render the 'system' open to the charge of 'mutilation of words' or 'disregard of the ordinary principles of language formation'?

For a negative answer to this question we need not look beyond the limits of the review itself, the language of which is pre-

sumed to be sanctioned by the authoritative journal in which it is printed. All of the following English words occurring therein differ from their Latin (or Latinized) antecedents in the omission of the inflected syllable: Form, system, barbarism, act, public, defect, subject, natural, official, distinct, historic, artificial, peculiar, human. If to these be added a few equally familiar, viz., arm, aqueduct, oviduct, tract, exit and stomach, it will be conceded, I trust, that *hippocamp* is in irreproachable etymologic company.

Indeed, we may now adopt the affirmative attitude and declare that among all the principles of language formation no one is better established or more generally recognized by scholars than that certain Latin words may be Anglicized by the elision of the ultima.*

I gladly forbear further direct and specific comment upon the case of *hippocamp*, but its more general aspects may be indicated in the three following queries:

1. Does scientific comity (which is comparable in some respects with what is called 'senatorial courtesy') render it incumbent upon the author of an article to refrain from disavowing responsibility for unjust statements wrongly attributed to him by a reviewer?

2. Should editorial regard for the privileges of writers tolerate the publication of unsound linguistic allegations that bring discredit upon American scholarship?

3. Is it probable that further assaults upon the simplified nomenclature from the etymologic standpoint will redound to the advancement of knowledge or the credit of the assailants?

*This is simply one of several well-known ways of converting Latin words into English; others are enumerated in 'Anatomical Terminology' (Reference Handbook of the Medical Sciences, VIII., 527); for all such processes of word-adoption the term *paronymy* (from *παρωνυμία*, the formation of one word from another by inflection or slight change) was proposed by me in 1885.

XXV. That, saving perhaps in the case of such German anatomists as read English with difficulty, the amount and nature of the information contained in the article numbered 5 in the note to p. 566 over and above what was already accessible to them in my own publications compensates for the misapprehensions likely to be occasioned by it.

XXVI. That efforts toward the establishment of an international nomenclature should be abandoned because of the arrogance of individuals or committees of particular nations.—As an evidence of the existence of a real discouragement in this respect I quote from a recent private letter from a well-known naturalist:

"I am not a believer in international coöperation, since it generally means that one nation has it all its own way."

If we read between the lines and recall the epigram, 'Man and woman are one, but the man is the one,' it may be imagined that my pessimistic correspondent adumbrates the doctrine, 'As to Anatomic Nomenclature all nations are one—but Germany is the one.'

XXVII. That, in estimating the probability of the soundness and eventual adoption of my terminologic proposals, there should be taken into account only or even mainly the terms that are new or otherwise less acceptable, rather than those respecting which my adoption antedates that of the *Anatomische Gesellschaft*.—Let us grant, for the sake of argument, that my *aula*, *porta*, *cimbria*, *mesocælia*, *metatela*, *metaporus* and the like are doomed to 'innocuous desuetude'; shall the folly of their vain introduction outweigh the evidences of sane prevision exhibited between the years of 1880 and 1895 in the deliberate and independent choice, among abundant and perplexing synonyms, of, for example, the following: *Pallium*, *gyrus*, *fissura*, *insula*, *centralis* (rather than *Rolandi*), *collateralis*, *calcarina*, *paracentralis*, *præcuneus*, *cuneus*, *hippocampus*, *fornix*, *thalamus*, *hypophysis*, *di-*

encephalon, tegmentum, vermis, nodulus, flocculus, pons, lemniscus, obex, oliva, clava and vagus?

XXVIII. *That the originality of the B. N. A. (the Nomenclature adopted at Basel in 1895 by the Anatomische Gesellschaft) is to be measured by the manifestation therein of non-acquaintance with what had been proposed or accomplished by English-speaking anatomists.—To be more explicit, I repeat here a paragraph from 'Neural Terms' (§ 276) referring to the action of American Committees between 1889 and 1892 :*

"Although the specific terms included in these recommendations are few, they exemplify all the commendable features of the German report. Indeed, I fail to discover in the latter any general statement, principle, rule or suggestion that had not already been set forth with at least equal accuracy, clearness and force in the writings of British and American anatomists prior to 1895."

XXIX. *That indifference or even hostility to terminologic improvement, especially upon the part of the older generation, should be thought either surprising or discouraging.—The first point was conceded by me in 1881 :*

"The trained anatomist shrinks from an unfamiliar word as from an unworn boot ; the trials of his own pupilage are but vaguely remembered ; each day there seems more to be done, and less time in which to do it ; nor is it to be expected that he will be attracted spontaneously toward the consideration that his own personal convenience and preferences, and even those of all his distinguished contemporaries, should be held of little moment as compared with the advantages which reform may insure to the vastly more numerous anatomical workers of the future."

The second point is covered by the review in the *Philadelphia Polyclinic*, which I have included in Category B (xxi.) :

"While some of our friends across the Atlantic may possibly consider this too radical a departure from long-established customs, the author of the book believes that time and familiarity with the terms will justify the course he has followed."

XXX. *That action upon the general subject should be indefinitely postponed.—This is the hour and you are the men. Let not the*

'fools rush in, because the 'angels' of this Association 'fear to tread.'

XXXI. *That it is incumbent upon this Association to decide immediately upon the names for all parts of the body or even for all parts of the central nervous system.—In a matter of such moment precipitation is to be avoided.*

XXXII. *That there are contemplated by the majority of the Committee, or by any member thereof, with regard to the names of the other parts of the body, changes comparable in number and extent with what have been proposed for the central nervous system.**

XXXIII. *That members of the Association should content themselves with simply awaiting the operation of the law of the survival of the fittest.—Upon this point I quote again the brothers Herrick. The conclusion of their article, 'Inquiries,' etc., reads:*

"The unification of our nomenclature is to be accomplished, if at all, by a process of survival of the fittest among competing terms at the hands of our working anatomists rather than by legislative enactment. Yet the international discussions now in progress may do much to further this end."

I trust they will pardon me for attaching the greater significance to the final concession. The subject before us is preëminently one that concerns mind rather than matter; and its determination should be reached not so much through the operation of numbers or force as by the exercise of the highest human qualities, deliberation, self-restraint, and consideration for others.

XXXIV. *That members of this Association should defer to what is called 'general usage.'—Of all so-called leaders, the most incapable, blundering, and dangerous is 'General Usage'. He stands for thoughtless imitation, the residuum of the ape in humanity; for senseless and indecorous fashions, the caprices of the demi-monde; for superstition and hysteria, the attributes of the mob; for*

* See, for example, the report submitted and adopted at this session ; SCIENCE, March 3, 1899, p. 321 ; also, *Phil. Med. Journal*, Feb. 25th, and *Jour. Comp. Neurology*, ix., No. 1.

slang, the language of the street hoodlum and of his deliberate imitator, the college 'sport'; and, finally, in science, for the larger part of the current nomenclature of the brain. As scholarly anatomists it is at once our prerogative and our duty to scrutinize and reflect, and to deal with the language of our science in the same spirit and with the same discrimination that we maintain in regard to the parts of the body and the generalizations concerning them.

It may be that a crisis has been reached; that this is the turning-point. If defeat awaits us, let there be no doubt as to my attitude. Let me be regarded as the chief offender, and let the group of terms advocated by me be derided as 'Wilder's Scientific Volapük.' But if, rather, despite errors and reverses, we are in the end to overcome inertia and prejudice, then I trust that the labors and sacrifices of so many English-speaking anatomists for the simplification of anatomic nomenclature may be recognized in the designation: 'The Anglo-American System.'

Indeed, whatever be the fate of any particular set of terms, of this I am assured: that system will ultimately prevail which is approved and used by anatomists of the English-speaking race—the composite, all-absorbing, expanding, dominating race of the future.

In no spirit of national self-glorification, much less with any personal animosity, but rather as a friendly injunction to prepare for the inevitable, I shall not object if portions of this address (for all of which, be it understood, I alone am responsible) are interpreted as a declaration of intellectual independence; as a claim for the recognition of what is done in England and America upon the basis of its intrinsic value; and as a protest against an indifference which in some instances has seemed to lack even that semblance of consideration which at least was commonly maintained during

the manifestation, a generation ago, of what an American scholar characterized as a 'certain condescension observable among foreigners.'

Let me conclude with a passage in more cheerful vein:

"When the first little wave of the rising tide comes creeping up the shore the sun derides her, and the dry sand drinks her, and her frightened sisters pull her backward, and yet again she escapes; and still her expostulating sisters cling to her skirts, and the rabble of waves behind cry out against her boldness, and all the depths of the ocean seem rising to drag her down. And now the second rank of waves, who would have died of shame at being the first, have unwillingly passed the earlier mark of the little wave that led them; and now you may float in your ship, for lo! the tide is full. So it is with all systems of reform; though the pioneers be derided, the great needs of humanity behind push on to triumphant acquisition of the new order of things."

BURT G. WILDER.

CORNELL UNIVERSITY.

THE BREEDING OF ANIMALS AT WOODS HOLE DURING THE MONTH OF SEPTEMBER, 1898.

WITH the month of September the record of the breeding habits of the summer fauna practically closes. Very few of the species continue to breed into October. The auftrieb, though less rich in species, is at the beginning of the month similar to that of late August, but after the first week the number of forms steadily decreases. It consists for the most part of crustacean larvæ, the bulk of the material being brachyuran and eupagurid.

The temperature of the water was constant at 72° F. for the first week. It then fell steadily until the 25th, when it reached 65° F., and remained at this point until the

close of the month. The density varied from 1.0208 to 1.0225.

Vertebrata. The fishes present no features of special interest, as the summer forms are still present, and no species is breeding. The surface skimmings show a few fry at intervals. On the 20th one or two larval flatfish, in which the eyes had just begun to migrate, were taken. About the 25th three smooth dogfish, *Galeus canis*, which had been confined in the 'Pool,' were killed, and twenty-seven embryos, 10 to 11 cm. long, were found in the oviducts.

Crustacea. None of the adult brachyura examined were breeding. Zoëæ were conspicuous in the auftrieb during the early part of the month, and later various megalops were abundant.

The zoëa of *Callinectes hastatus* was the most abundant form, lasting about two weeks. The megalops of this species was plentiful at all times, particularly on the 18th. Specimens in the laboratory changed to the beautifully-spotted 'first adult' on September 27th, 29th, and October 3d. Another zoëa (which I have not identified) was very abundant in the latter part of August and the first week of September, disappearing about the 11th. It resembles the zoëa of *Callinectes*, but has a longer rostrum and dorsal spine, and the exopodite of the antenna is a straight blade as long as the rostrum.

Among the Anomura, the larvæ of *Hippa* had disappeared on September 4th. Eupagurid zoëæ swarmed in August and the first week in September, and were present in decreasing numbers throughout the month. The 'glaucothoë-stage' was abundant at all times. Data relating to the breeding of *Eupagurus bernhardus* and *E. pollicaris* are scanty, but the few females of the latter species which were examined were without eggs. *E. annulipes* was brought in on the 4th, when a few were bearing eggs in early stages of development. Females of

E. longicarpus with eggs were taken as late as the 13th.

Among the Macroura, specimens of *Virbius zostericola* had eggs in the later stages on the 11th. Larvæ and young adults, ranging in length from 5 mm. to 10 or 15 mm., were present in the skimmings. Those of small size persisted throughout the month. *Palæmonetes vulgaris* was not breeding, but the larvæ (mostly the 'fifth' and 'sixth' stages of Faxon) were occasionally taken, and toward the end of the month several of the 'first adult stage' were found. A specimen of *Crangon vulgaris* with eggs was obtained on the 19th. *Heteromysis*, dredged at Vineyard Haven on the 12th, and off Nobska Point about a week later, had well-advanced eggs in the brood-pouch.

No adult Isopoda were examined, but immature *Idotea robusta* and *I. irrorata*, ranging in length from 2 mm. upward, frequently appeared in the skimmings.

Among the Amphipoda, a minute form, apparently a species of *Montagua*, was very common among the hydroids. On September 21st nearly all were carrying eggs in various stages of development. Many *Caprellæ* obtained at the same time bore embryos approaching maturity.

Squilla larvæ (5 mm. long) appeared at intervals throughout the month. Copepods were abundant at all times. *Diastylis* was taken in the evening and is apparently attracted by any artificial light. On the 12th a number of 'Goose Barnacles' had eggs in all the later embryonic stages, and some began to liberate nauplii about this date.

Mollusca. *Scycotypus* continued to deposit its 'egg-strings' during the first two weeks of the month. The breeding period of *Crepidula fornicata* had closed, but on the 19th I found a few specimens of *Crepidula plana* with eggs in early cleavage stages. The breeding period of *Littorina littorea* in American waters is not known. On the 20th great

numbers of young, about one millimeter in diameter, were found on the rocks at Nobska Point. During the latter part of August and the early part of September, Veligers, all apparently of one species, were conspicuous in the surface skimmings; these disappeared at about the time that the young *Littorina* were found.

Vermes. Mr. R. H. Johnson found *Bugula turrita* liberating embryos, even after the middle of the month.

Small specimens of *Nereis limbata* and certain allied forms occurred sparingly in the auftrieb. On the evening of the 30th *Autolytus* was still fairly abundant, and many of the females were carrying eggs in early stages of development. *Rhyncobolus* and *Diopatra* were not breeding.

Cœlenterata. With the exception of one or two minute forms, no Medusæ were found. *Gonionemus* was abundant in the Eel Pond, and specimens brought into the laboratory about the middle of the month extruded eggs. The greater part of these eggs did not reach the blastula, and none developed beyond this stage. Ctenophores *Mnemiopsis*, very conspicuous in late August, appeared in increasing numbers during September. *Obelia*, with a few ripe gonangia, was obtained on the 21st. *Pennaria tiarella* formed the bulk of the abundant hydroid-growths on the Fish Commission wharves, although a *Eudendrium*, probably *E. ramosum*, was plentiful. Here and there small patches of *Plumularia tenella* were found. East Chop and Edgartown were visited on the 12th. At the former place there were few colonies of *Pennaria*, but a great abundance of *Eudendrium* and *Plumularia*. At Edgartown I did not find either *Pennaria* or *Eudendrium*, but *Plumularia* occurred in dense masses, which literally covered the submerged woodwork of the wharves.

At Woods Hole the colonies of *Plumularia* were small and sterile, while at the other localities they were large and provided with

gonangia in the various stages of development.

The *Eudendrium* and *Pennaria* bore medusa-buds in all stages, and the latter species remained in fruit as late as the 21st, and perhaps later.

M. T. THOMPSON.

ECONOMICS IN MANUFACTURES.

ONE of the most difficult problems in practical economics, in the whole range of modern industrial systems, is that of securing a just and satisfactory method of insuring fair exchange of labor for capital or wages where large bodies of workmen are to be employed. Cooperation and innumerable plans of 'piece-work' and 'profit-sharing' have been proposed, and none have, in practice, been found either in the abstract entirely equitable or wholly satisfactory to the employer as securing sufficient output from his always burdensome investments, profit on his sales, or a contented and fair-minded relation between himself and his employés; nor has any system been found which fully satisfies the workman in either extent of total compensation, opportunity to secure compensation proportioned to his exertions and ability, or in abstract equity in distribution of profits.

One of the most promising of the later plans for a fair and honest and satisfactory distribution of profits and a very effective stimulus of the right spirit in both employer and employé was described, as a first experiment, to the American Society of Mechanical Engineers, some years ago, by Mr. F. A. Halsey, then or earlier manager of the Canadian Rand Drill Co., at Sherbrooke, Quebec, Canada. Mr. Halsey called his plan 'The Premium Plan of Paying for Labor,' and the title is indicative of its nature.*

The author of this system now reports the outcome of a considerable number of

* Trans. Am. Soc. Mech. Eng'rs; Vol. XII.

experiments in its employment, some by important and famous manufacturers of various mechanical devices, from the steam-engine to the machine-tool. The following abstract is based upon his account of these later experiences, as given in the *American Machinist*, with extended tables of data and results.*

The plan has been in use eight years, and has come into use, in a number of establishments, sufficiently to give ample experience in its workings. Curiously enough, however, although devised for the benefit of the workmen, mainly and primarily, and invariably promising them gain, it has as invariably been received with suspicion and reluctance by them, and in at least one case has been opposed by the trade-unions of the place. In all but a single case, however, it has proved entirely successful in the accomplishment of its purpose—the promotion of the wage-earning power of the men and of the dividend-paying power of the establishment; sharing profits while stimulating ambition and increasing output. It gives the workman increased day's wages; it gives the employer increased output from his works, at reduced cost and increasing profits, shared with those who make them possible. The workman gains directly, day by day; the employer not only gains, directly, by increased output from the same number of men, but also indirectly and in an exceedingly important degree, often, through the increased earning power of his capital, invested in plant and in funds.

Piece-work has not been wholly successful, and in too many cases the selfishness and greed of the employer, seeking to monopolize all the profit, compels the workman to accept a rate which makes his day's work no more profitable to him when working under high-pressure than when doing an ordinary day's work at fixed wages at such a rate that he can sustain that amount

* March 9, 1899.

of production indefinitely. Where properly adopted and adjusted, it is a vast improvement upon the older plan. Mr. Halsey's plan puts a premium upon increasing production, in such manner that both employer and employé are inevitably alike advantaged, and skill and industry and steady work secure proportional reward. It involves something of the principle of the common bargain by which a salesman is given a fixed and moderate salary *plus* a stated percentage on sales. Under this new plan the employer offers a workman a premium, perhaps ten cents, for each hour by which the production of a certain piece is reduced below that of the observed normal average or below an assumed period of time; the day's wage being that of the time and place, as fixed by ordinary circumstances in the market, and without control, usually, by either party to the bargain.

Suppose that pay to be three dollars a day and an hour to be saved in a piece ordinarily requiring just a day's work for its production. The proprietor gains the hour and his thirty cents otherwise paid as wages for the hour; he loses ten cents premium; he gains in rate of output of the establishment, and so makes it possible to secure larger returns through more effective use of all other capital than the 'wages fund.' The workman gains his ten cents and the privilege of adding an extra hour's work on a new 'job.' Thus both parties gain. Had the premium been fifteen cents the money-gain would have netted both equal amounts, fifteen cents, per day. Thus, as in Table I., we sometimes actually find enormous gains possible through the ingenuity of the workman in finding ways of reducing time of production, as by increased personal activity, or by securing deeper cuts and higher speed of cutting, or less time in putting the piece in place or in replacing it by its successor, etc. The writer has known of a case in which the

cost of an important machine was reduced by such expedients from \$250 to about \$75.

TABLE I.—OPERATION OF THE PREMIUM PLAN.

1	2	3	4	5
Time consumed.	Day wages per piece.	Premium earned per piece.	Total cost of work = col. 2 + col. 3.	Workman's earnings per hour = col. 4 ÷ col. 1.
Hours.				
10	\$3.00	\$0.	\$3.00	\$0.30
9	2.70	.10	2.80	.311
8	2.40	.20	2.60	.325
7	2.10	.30	2.40	.343
6	1.80	.40	2.20	.366
5	1.50	.50	2.00	.40

Table II. is taken from the books of one of this establishments actually employing this 'premium plan,' and shows a gain of more than one half, in this particular instance, in time of production—in productivity, in fact—in the works, of just double wages for the workman, per piece produced, and a net increase in day's wages of eighteen per cent.; while the gain to the company was very much greater through its operation upon the interest and maintenance accounts.

In another actual case where the parts reported upon all belonged to a single contract, and comprised the whole contract, the gains of the workmen were 29 per cent. on the day's wages, 25 per cent. on the piece, and the time of production of each piece averaged a reduction of 63 per cent. These figures are astonishing; but they mark the enormous difference between the productivity of a man working under the old conditions of the day's-work plan, without incentive to either good work or to doing his best in continuous labor, and the premium-system, which is likely to give ambition, energy and productiveness to the most stolid. In this table Cases 41 to 44 are records made where both parties doubted the possibility of any gain at all. One case was made by an apprentice boy and the standard was based on the work of an experienced workman. Another case gives illustrations of successive gains with practice on successive pieces. All illustrate large and equitably-shared gains over the old system of day's wages.

Everything depends, however, upon an equitable basis of inauguration. It is bet-

TABLE II.—RESULTS OF APPLYING THE PREMIUM PLAN TO MISCELLANEOUS WORK.

RATIOS OF TOTALS.

New time = $\frac{44}{100}$	New wages per piece = $\frac{50}{100}$	New wages per day = $\frac{118}{100}$
Old time = $\frac{100}{100}$	Old wages per piece = $\frac{100}{100}$	Old wages per day = $\frac{100}{100}$

RATIOS OF TOTALS WITH 46 OMITTED.

New time = $\frac{76}{100}$	New wages per piece = $\frac{88}{100}$	New wages per day = $\frac{118}{100}$
Old time = $\frac{100}{100}$	Old wages per piece = $\frac{100}{100}$	Old wages per day = $\frac{100}{100}$

Note that while this table deals with small parts it also deals with large lots. The ratios at the bottom compare the production of over 900 pieces by each method.

NATURE OF WORK.	Operation	DAYS WORK PLAN		PREMIUM PLAN		Old wages cost per piece	New wages cost per piece	Old wages per day	New wages per day
		Time per piece	No. of pcs. in lot	Time per piece	No. of pcs. in lot				
		Hours		Hours					
41. Long T-shaped piece cast-iron..	Chuck, Drill & Ream	.125	300	.089	300	\$.0275	\$.0258	\$2.20	\$2.82
42. " " " larger size cast-iron	" " " "	.178	200	.112	200	.0492	.0317	2.20	2.84
43. " " " " " " " "	" " " "	.275	100	.175	100	.0605	.0533	2.20	3.06
44. " " " largest " " " "	" " " "	.366	100	.183	100	.0805	.0624	2.20	3.41
45. Cast-iron wedge.....	Plane	3.5	2	3.25	4	.805	.792	2.30	2.44
46. Box-shaped casting.....	Oblique planing	56.	1	21.	1	14.00	6.25	2.50	2.98
47. Cast-iron wedge.....	Plane	3.75	2	2.62	4	.825	.727	2.20	2.76
48. Small pulleys, cast iron.....	Chuck Drill & Ream	.18	100	.13	100	.045	.0417	2.50	3.20
49. Spindle steel.....	Grind 3 sizes	.6	50	.36	50	.144	.09	2.40	2.50
50. Small head stock.....	Mill 3 operations	.45	50	.32	50	.1237	.0925	2.75	2.90
Totals	65.424	28.239	\$16.1604	\$8.1664	\$24.45	\$28.91
Totals omitting 46	9.424	7.239	\$2.1604	\$1.9164	\$21.95	\$25.93

ter for the employer to be liberal in estimating the time-rate rather than with the premium-rate. Excessive premium-rates are apt to result in too large expectations to be fully met in the long run. From one-half to one-third the saving are usual premium-rates, and probably one-third to the workman and two-thirds to the firm best brings out a permanent and satisfactory adjustment which, if found inequitable, can generally be easily readjusted to a correct figure. In one machine-tool works the premium-rate is thirty-six per cent. and is found satisfactory to both sides. The higher premium-rates, however, should be paid for manual labor, as in blacksmithing, and the lower to power-tool work, as at the lathe or the planer or the milling machine. Undoubtedly every establishment, and every department of labor, from floor-sweeping to book-keeping, has its own peculiar best rate. In all cases the result may be expected to be a largely increased output of the works, a greatly increased earning power on the part of the men, and decreased costs of production with increased dividend-paying power for the holders of the capital. "Wisely administered, the plan will do more to settle the wages-question than anything else that has been suggested," and the wages-question is to-day the burning question in the economics of manufacturing.

R. H. THURSTON.

SCIENTIFIC BOOKS.

Analytic Functions. Introduction to the Theory of Analytic Functions. By J. HARKNESS and F. MORLEY. London, Macmillan & Co. 1898 8vo. Pp. xvi + 336.

The appearance of the present work is a very pleasant sign to friends of the modern school of mathematics in England and America. It indicates that the movement which set in some years past with us in this direction has been steadily growing; that the theory of functions is no longer the property of a few bold and rest-

less minds, but has already descended to the masses. The present work may very happily serve as a text or reference book to a first course on the theory of functions in the senior class of any of our better universities. The theory of functions of a complex variable may be viewed from two standpoints. One was taken by Cauchy and Riemann; the other by Weierstrass. The methods of Cauchy and Riemann are more natural and intuitive; those of Weierstrass more abstract and lend themselves more easily to a rigorous treatment of the subject. The authors have chosen the methods of Weierstrass.

Roughly speaking, the subjects treated in the first 100 pages fall under two heads:

1. The geometric representation of complex numbers, the conformal representation afforded by

$$y = \frac{ax + b}{cx + d}$$

and the first properties of rational functions.

2. Topics which lie at the foundation of the calculus.

The treatment of the first group of subjects is admirable. In regard to the second it seems to us that the authors have attempted the impossible. The theory of function in common with the calculus rests on certain notions, such as that of number, limit, continuity, extremes of functions, etc. These subjects are very imperfectly treated in English works on the calculus, and our authors have thus found it advisable to give some account of them in the present volume. The amount of space at their disposal was very limited, and they have, therefore, been obliged to be excessively concise. This has been carried to such an extent in the chapter on number, Chapter I., that the subject, so it seems to us, will be utterly incomprehensible to the student.

We cannot understand why, if it is worth while to say anything about irrational numbers, the arithmetical operations upon them are passed over in absolute silence. Until the terms sum, product, etc., are defined they have no meaning.

Chapter VI., which treats of limits and continuity, suffers severely on account of the brevity of Chapter I. In this chapter it is important to establish the existence of certain

numbers. The arguments cannot have much meaning to the student until the material of Chapter I. has been grasped, and this seems out of the question.

Before leaving this section we call attention to a curious break. On page 48 complex functions of a real variable are differentiated and integrated. This certainly is illogical until such operations have been defined. We are tempted to believe that the beauties of this chapter will fall very flat with the average student. If the geometrical theory of the logarithm is to appeal to him, what is stated here so rapidly should be given with leisure and detail.

The next 60 pages, Chapters VIII.-XII., deal with infinite series, and so lead us to Weierstrass's conception of analytic functions. This, as is known, depends on infinite series ascending according to integral powers of $(x-a)$. The treatment here is very superior—the authors show a masterly grasp of the subject. A short chapter on the analytic theory of the exponential and logarithmic function now follows.

Chapters XIV. and XV., pp. 178-209, turn again to the general theory. Singular points are discussed, and Weierstrass's decomposition of a function into prime factors is deduced. Application is made to show that

$$\sin \pi x = \pi x \prod (1 - x^2/n^2). \quad n = 1, 2, \dots, \infty.$$

The consideration of the zeros gives at once

$$\sin \pi x = x e^{G(x)} \prod (1 - x^2/n^2).$$

The determination of the integral transcendental function G is singularly difficult. It seems a pity that the method invented for Cauchy for the same purpose and which may easily be made rigorous is to-day quite neglected. By this method G is readily found.

With Chapter XVI., which treats of integration, we arrive at the starting point of the Cauchy-Riemann theory. It seems to us that our authors have not maintained the high ideals here as well as elsewhere. In a passage, pp. 11, 12, we read: "But in using geometric intuitions * * * we must emphasize one lesson of experience; that the intuitional method is not in itself sufficient for the superstructure. It has been found that only by the notion of number * * * can fundamental prob-

lems be solved. If, however, we are prepared to replace when occasion arises these geometric intuitions * * * then and only then is the use of geometry thoroughly available." It is true that the authors *here* speak of points, distances and angle only, but these remarks apply with equal cogency, as they will be the first to admit, to all geometric intuitions when used in analysis. We are, therefore, surprised to find the obscure notion of curve, of its length, of a closed curve, of a region, etc., freely used without any attempt to put them on a number basis. Such statements as that on p. 189, viz.: that a circuit divides the entire plane into two regions will certainly embarrass the authors to prove in its generality. Again, on p. 213, we see the authors implicitly define the length of a curve C to be $\int_C |dx|$. This definition differs from the one given our text-books, viz.: $\int dx \sqrt{1 + f'(x)^2}$.

As our authors propose to use a broader definition than usual, it seems only fair that they state this to the reader. Still a more serious objection is to be urged to their procedure. It results in stating Cauchy's fundamental theorem and other important theorems of this chapter without any restriction regarding the path of integration. This seems to us like talking of infinite series without bothering ourselves about convergence.

Chapter XVII. brings a brief discussion of Laurent's and Fourier's series. Then follow two excellent chapters on the elliptic functions. These are followed by two chapters or about 30 pages devoted to Algebraic functions and Riemann surfaces.

It appears to us that the fictitious number and point ∞ has been treated too hurriedly. These notions are very important and also difficult for the student to master. Our authors have followed the usual custom of disposing of them with a few words here and there. We believe the custom of introducing the number ∞ is bad. The theory of functions of a complex variable is a theory of two very special real functions of two real variables. In the theory of functions of real variables the number ∞ does not exist. It seems to us that its introduction can only produce confusion and embarrassment.

It is not a number ∞ we are ever concerned with. When we say $w(a) = \infty$ we really mean $\lim |w(z)| = \infty, x = a$. Again when we ask how does $w(z)$ behave for $z = \infty$ we really mean how does $w(1/\epsilon)$ behave in the vicinity of $\epsilon = 0$ where $\epsilon = 1/z$. Thereby ϵ is never required to assume the value of 0. On using the sphere instead of the plane we get the punktierte Kugel. The missing point we can supply or not at our option. In any case no number shall correspond to it. We firmly believe that the easy intuitional way of treating ∞ in the function theory of a complex variable must be modified as here indicated.

The last chapter is devoted to a brief *aperçu* of the function theory from the standpoint of Cauchy and Riemann. We cannot appreciate the difficulties mentioned in § 164 as underlying the definition of a function from the Cauchy-Riemann standpoint. They seem to us to be due to the belief on the part of the authors that we must take the whole z -plane into our definition from this point of view. Such is not the case. As a domain D for the variable z we take any point multiplicity consisting only of interior points. If it be possible to pass from any point of D to any other of it along a continuous curve $x = \phi(t), y = \psi(t)$ we say D is a simple domain. Otherwise D is composed of simple domains $D = D_1 + D_2 + \dots$. To get a synectic function $w(z)$ for D we take two single valued functions $u(x, y), v(x, y)$ defined over D and such that for every point in D they have a total differential and satisfy the equation.

$$\frac{\partial u}{\partial x} = \frac{\partial v}{\partial y} \quad \frac{\partial u}{\partial y} = -\frac{\partial v}{\partial x}$$

In any one of these simple regions as R_a , $w(z)$ can be developed into an integral positive power series. The analytic function $f(z)$ obtained from one of these elements is identical with $w(z)$. There certainly is no reason to suppose that $f(z)$ when continued into another region R_β should be identical with $w(z)$ in this region. This seems to answer all the objections in I and II of this article. Indeed, the advantage seems to be decidedly on the side of Cauchy, for exactly one of the points urged against Cauchy's theory is now without force, while it is, indeed, an important matter from Weierstrass'

standpoint. This, in the author's words, is: "That Cauchy's definition implies in various ways a considerable preliminary grasp of the logical possibilities attached to the study of singular points." From our standpoint we fix in advance the domain D ; it has no more singular points than we choose to assign. Not so with the analytic function. Here an element is given, one singular point must lie on its circle of convergence. Where the others are is a subject of further study.

We cannot see the difficulty mentioned under III. It is, indeed, an interesting matter to know 'the irreducible minimum of conditions to impose on $w(z)$,' but it seems to us nowise necessary. It suffices that we know the necessary and sufficient conditions in order that $w(z)$ can be developed according to Taylor's Theorem. This we know and we have taken them into our definition of $w(z)$. It may be interesting to remark, however, that these conditions are already known, as will appear in a remarkable paper of E. Goursat shortly to be published.

We close, congratulating the authors for writing a work which we believe will prove an excellent aid to acquire some of the essentials of the theory of function. We should have preferred to see the two theories of Cauchy and Weierstrass blended together into an organic and indivisible whole. Although these two theories grew up quite distinct, they have already been welded into one greater and more powerful theory. It is only the purist who still tenaciously clings to the methods of Weierstrass. It seems, therefore, very desirable to us that an introductory work should be written more in accordance with this fact.

JAMES PIERPONT.

YALE UNIVERSITY, March, 1899.

A Handbook of Metallurgy. By DR. CARL SCHNABEL. Translated by HENRY LOUIS. New York, The Macmillan Company. Two volumes, medium 8vo. Total pages, 1608. Illustrated. Volume 1, copper, lead, silver, gold. Volume 2, zinc, cadmium, mercury, bismuth, tin, antimony, arsenic, nickel, cobalt, platinum, aluminum. Price, \$10.00.

The author states in the preface that, while many exhaustive works have appeared on the

metallurgy of individual metals, the few books on general metallurgy were arranged as text-books and made no pretence of thoroughness of detail or treatment. With these facts in mind the present work was compiled, with the stated object of giving a complete account of the metallurgical treatment of every one of the metals ordinarily employed, together with the recent improvements in the art, stating the underlying scientific principles and illustrating by actual practice.

This object is highly commendable, but the statement is rather misleading, as iron and steel have been entirely omitted and no mention made of the omission or of a subsequent volume upon this all-important branch of metallurgy. This fact should have been stated plainly by the author in the preface and by the publisher in the advertisements.

Dr. Percy's historic work was selected as the basis, and on this are grouped many facts from the works of modern writers, notably Hofman, on lead; Peters, on copper; Egleston, on gold and silver, and Borchers on electro-metallurgy. The work is quite exhaustive in character, as the grand total of 1608 pages indicates, but, unfortunately, the exhaustion is not limited to the subject-matter of the book and oftentimes extends to the reader, as much of the material is vague and unnecessarily verbose. The work lacks that clearness of description, lucidity of arrangement and conciseness of statement so needful in the treatment of a large subject and so appreciated by American readers with whom time is an object.

It is to be regretted that much ancient material is perpetuated in excruciating detail, particularly as it is so interwoven with modern practice that the general reader is left in doubt what is in use at the present time. To illustrate this, under the chapter on silver, barrel amalgamation is quoted as now in use at the Pelican Mill, Georgetown, Colo., while, as a matter of fact, it was there abandoned twenty years ago. Another instance, under the chapter on zinc, the furnace used in the old English process—that rare bird of antiquity—shows forth resplendent in full detailed illustration. As to this furnace, Dr. Percy, in 1869, failed to find even the ruins of its foundation.

The large amount of material collected in these two volumes contains much of value to the specialist, but it is too encyclopedic in character to be of any marked assistance to the general reader. Its main value is for reference in a scientific or technical library.

A few minor errors, such as the location of Boston in Vermont (Vol. 1, p. 115) and Orford in New Jersey (Vol. 2, p. 104), may be overlooked in a work of this large size.

The criticism of this work may be considered harsh, but the eminent position occupied by Dr. Schnabel leads one to expect the highest standard of work and to be disappointed if it is not attained.

J. STRUTHERS.

BOOKS RECEIVED.

Organic Chemistry. Edited by R. ANSCHÜTZ. Authorized translation by EDGAR F. SMITH. Vol. I., Chemistry of the Aliphatic Series. Philadelphia. P. Blakiston's Son & Co. 1899. Pp. xviii + 625. \$3.00.

Commercial Organic Analysis. ALFRED H. ALLEN, Philadelphia, P. Blakiston's Son & Co. 1899. Vol. II., Part I. Pp. x + 337. \$3.50.

The Spirit of Organic Chemistry. ARTHUR LACHMAN. With an introduction by PAUL C. FREER. New York, The Macmillan Company. 1899. Pp. xviii + 299. \$1.50.

The Arithmetic of Chemistry. JOHN WADDELL. New York and London, The Macmillan Company. 1899. Pp. viii + 133. 90 cents.

Allgemeine Erdkunde. J. HANN, ED. BRÜCKNER and A. KIRCHHOFF. III., Abteilung Pflanzen- und Tierverbreitung. ALFRED KIRCHHOFF. Prague. Wien und Leipzig, F. Tempsky. 1899. Pp. xi + 327.

SCIENTIFIC JOURNALS AND ARTICLES.

The Botanical Gazette for March contains the following papers: D. H. Campbell: 'Notes on the structure of the embryo-sac in *Sparganium* and *Lysichiton*,' pp. 153-166, with one plate. This is a continuation of the author's studies of the primitive monocotyledons. The discovery of special interest is the extraordinary development of the antipodal cells in *Sparganium*, another evidence of the variable nature of the antipodal region. H. C. Cowles: 'The ecological relations of the vegetation on the sand

dunes of Lake Michigan,' pp. 167-202, with eight photographs. This very complete ecological study of the dune floras is continued from the February number. A special feature of this part is the discussion of embryonic dunes. The active or wandering dunes are also taken up and will be completed in a subsequent number. The following briefer articles appear: Ralph E. Smith: 'A new *Colletotrichum* disease of the Pansy;' E. J. Hill: 'A new biennial-fruited oak,' with two plates; Elias Nelson: 'The Wyoming species of *Antennaria*,' in which eight new species are described. Numerous Book Reviews and Notes for Students complete the number.

SOCIETIES AND ACADEMIES.

ANTHROPOLOGICAL SOCIETY OF WASHINGTON.

THE 289th regular meeting of the Anthropological Society was held Tuesday, March 28, 1899. Dr. J. Walter Fewkes made a communication on the 'Winter Solstice Altars at Hano,' a Tewan pueblo in Tusayan. He began by saying that the Territory of Arizona is covered with mounds or ruins indicative of the habitations of prehistoric pueblo people, but that it is evident that these villages were never simultaneously inhabited. Their distribution shows that this agricultural, aboriginal population of Arizona was more evenly distributed over the Territory in ancient times than at present. The presence of nomadic enemies—Utes, Apaches, Navajos and others—had led to a concentration of the pueblo aborigines of this region into limited areas, a movement which began in the 15th century and was continued in the two following. The so-called province of Tusayan was one of those centers of concentration or refuge, and the inhabited pueblos of the area now contain some of the descendants of the survivors of the abandoned villages between the Mojollones Mountains and the Utah boundary.

Three of these Tusayan pueblo—called Walpi, Siteomori and Hano—are situated on one mesa, not more than a gunshot apart. Dr. Fewkes showed how Walpi had been founded by clans driven southward from the Colorado River, and how their pueblo had grown by successive incoming clans from south and east. At the end of the 17th century the hostile nomads had so

closed in on Walpi that they swarmed in their farms, and utter annihilation stared the Hopi in the face. The Governor of Walpi sent to New Mexico for help, and after four appeals a band of Tewa warriors from a pueblo in the upper Rio Grande valley went to his aid. These warriors drove back the Utes, and in return for this help, the Tewa were given a site for their home near the main trail to the mesa upon which Walpi is situated. The village which they built is now called Hano. For two centuries the successive generations of inhabitants of Hano have remained Tewan in their customs in the country of their adoption. Hano preserves the Tewan language, although, by marriage with the neighboring Hopi, the consanguinity of the inhabitants is more Hopi than Tewa. Similarity of language is not always a sign of blood kinship. There are also many Tewan customs in marriage, mortuary and other rites in Hano, but the most characteristic of all are the religious festivals. The most instructive of these are the winter-solstice rites.

Of all expressions of religious sentiment objects like fetishes and ceremonial paraphernalia are the least variable from generation to generation. Mythology changes as man advances in culture or lives in a new environment, and accretions in form of myths to adjust worship to the spirit of the times multiply from generation to generation. Expression of the religious feeling through acts or dramas called ceremonies is more conservative than through myth and less modified by the evolution of culture, and new myths are invented to harmonize and explain ceremonies handed down from ancient times. The objects used in worship—fetishes, idols, paraphernalia—change even less than rites or myths, and reflect better than both the true ancient religious sentiment of which they are expressions, and are, therefore, of preeminent importance to the ethnologist in the study of ethnographic religion.

These ceremonial objects are very numerous among the Hopi; and their installation in sacred rooms, at times of great ceremonies, is called an altar. The two altars at Hano during winter-solstice rites were described in detail. The most striking fetishes upon them were clay images of the Great Snake. There were also

rain-cloud symbols, gaming implements, water-worn stones, puma paws and other objects. The imitation of an ancient ladder which stood back of the altar was called a sun-ladder, and was interpreted as a symbolic aid to the sun, who is supposed to be weary at the winter solstice. Through sympathetic magic he is thus supposed to gain strength to mount the sky from his home at sunrise.

These altars at the winter-solstice ceremony in Hano made it possible to know something of the character of the ancient Tewan Sun and Snake worship, of which little has yet been recorded, although this pueblo stock has been, and still remains, one of the most important in the upper Rio Grande pueblos. Possibly studies of secret rites in the estufas of the latter will bring to light the characteristics of their winter-solstice altars, but it is also possible that these altars have been abandoned, in which case the survivals at Hano, described by Dr. Fewkes, have value in a comparative way, as indicating the nature of Tewan altars in mid-winter.

Mrs. Olive Ennis Hite presented a paper on 'New Mexican Folk-Lore,' in which she described the environment of these people and showed the influence it had upon their superstitions. Their belief in the 'Hombrecito,' or little brown people, was widespread, and it was considered lucky to see one of these creatures, who were visible to the 'pastores,' or shepherds, only. Of 'las brujas,' the witches, there is less said, and that little with many 'carambas' and audible supplications for the intervention of 'la Santissima Maria.'

Discussed by Drs. McCormick, Fewkes and Kober, Professor McGee, Dr. Wilson, Mr. Pierce and Miss Alice C. Fletcher.

J. H. McCORMICK,
Secretary.

GEOLOGICAL CONFERENCE AND STUDENTS' GEOLOGICAL CLUB OF HARVARD UNIVERSITY.

Students' Geological Club, March 14, 1899. Mr. A. W. Grabau reviewed the paper which Professor Shaler has recently published on the Geology of Cape Cod (18th Annual Rep., U. S. Geol. Surv.). The speaker did not agree with the view advocated by Professor Shaler, that the topography of lower Cape Cod, from

Orleans to Highland Light, is mainly erosional and scarcely modified by ice action. But he held that the orientation of the valleys, the character of the slopes, and the presence of typical kettles all over the cape, indicate that most of the material of Cape Cod is of glacial origin.

Geological Conference, March 21, 1899. Mr. F. M. Buckland gave a paper on 'Winter Changes about Fresh Pond.' After briefly reviewing the literature on the expansion and contraction of ice on water bodies, he described some of the effects of these agencies on the shore of Fresh Pond during the past winter.

Mr. J. B. Woodworth presented some results of field observations on 'Moen's Cliff and the Maars of the Eifel.' The Cretaceous and Pleistocene beds of the island of Rügen, off the coast of Germany, and Moen, off the coast of Denmark, show a disturbance which is comparable in degree and character to that in the Cretaceous and Pleistocene of Martha's Vineyard. H. Credner attributes this deformation to the shoving action of an ice sheet which was immediately previous to the last. A few other geologists favor purely orogenic agencies. In neither case has conclusive physical evidence been found. The lantern views, which are recent accessions to the Gardiner Collection, illustrated this deformation and related features, and the Weinfelder and Gemündener Maars near Daun.

J. M. BOUTWELL,
Recording Secretary.

TORREY BOTANICAL CLUB, FEBRUARY 28, 1899.

PROFESSOR L. M. UNDERWOOD presented a paper on 'Species confused under the name *Aspidium juglandifolium*,' discussing the characters and geographical district of the forms regarded by him as distinct species, eight in all, constituting the whole number attributed to the genus *Phanerophlebia*. He remarked in concluding that it would be unsafe to describe new species without consulting the valuable collections of ferns in Europe, and especially at Kew. The paper will appear in the *Bulletin*.

Miss Alice Lounsberry then exhibited the very valuable collections of flower paintings by Mrs. Ellis Rowan, which constitute the origi-

nals of the colored plates in Miss Lounsberry's forthcoming work, 'How to Know the Wild Flowers.' Selections which showed the character of the book were read, including the Introduction, written by Dr. Britton, and the Preface, which pointed out the fact that the distribution of plants according to soils was made the keynote of the work.

Dr. Britton said that the book was interesting to him on two accounts, from the ecological basis of classification and the remarkable reproductions in color.

In the absence of Mrs. Annie Morrill Smith, of Brooklyn, Mrs. E. G. Britton read for her the manuscript of a paper, entitled 'The Flora of the Adirondack Mountain Club Area.'

Meeting of March 14, 1899.—The Summer Courses in Botany given jointly by this Club and the College of Pharmacy were announced to begin at 4:30, March 24th, ending June 10th, the General Course to be given by Dr. H. H. Rusby, that in Histology by Dr. M. A. Howe.

The paper of the evening, by Mrs. Caroline A. Creevey, on 'Plant Juices and their Commercial Values,' described the secretions, oils, gums, resins and other products of plants, with exhibition of numerous specimens. Among the numerous oils considered none has become so important commercially as cotton-seed oil, now produced at about 28 million gallons per year, pressed from 800,000 tons of cotton seed. Another industry dependent upon plant juices is that of tanning, the tannin found in the saw-palmetto and in *Rumex hymenosepalus* promising to revolutionize the process of the leather-industry. The waste sands occupied by these plants in the South and West bid fair to become valuable.

Dr. Underwood exhibited a series of photographs of the Fleshy Fungi by Mr. G. A. Anderson, of Lambertville, N. J., colored from the living specimens by his daughter, Miss H. C. Anderson. They illustrate a new process of preserving fleshy fungi.

Dr. Britton reported a brief communication from Mr. A. A. Heller sent from Porto Rico, February 18th, reporting collections made about Ponce, Ibonito, Coamo, etc., now reaching 584 numbers after six weeks' work. On the north side of the islands many species occur on the

shore which are montane species when growing on the south side.

Dr. Britton also read from a letter of February 26th, just received from Mr. S. Henshaw, from San Juan, describing the sugar plantations, now in the midst of cutting and boiling. He finds the flora not so varied as in Trinidad; the woods are few; in 100 miles he did not see a single large tree.

EDWARD S. BURGESS,
Secretary.

DISCUSSION AND CORRESPONDENCE.

DUPLICATION OF GEOLOGIC FORMATION NAMES.

REFERRING to Mr. F. B. Weeks' letter on this subject in your issue of March 13th, I venture to doubt whether Cache Valley group (1879) or Cache Lake beds (1888) can properly be considered as conflicting with each other or with the name Cache Creek formation. If, however, regarded as an undesirable duplication of similar names, I wish to point out that the Cache Creek group or formation undoubtedly holds priority, a circumstance which would scarcely appear from Mr. Weeks' remarks.

The name was first applied (by Dr. Selwyn, in 1872) as Upper and Lower Cache Creek groups, to certain rocks in British Columbia. The age of the upper series was only conjectured, but the lower was known to occupy a position somewhere 'between the base of the Devonian and the summit of the Permian.' In 1876 Carboniferous fossils were found in rocks assigned to the lower group in the northern part of British Columbia, and in the following year a re-examination of the original area led to the discovery of similar fossils in both lower and upper groups there. In my report for 1877 these groups are, therefore, referred to collectively as the Cache Creek series. In the latest report dealing with these rocks the same usage is followed, although upper and lower parts of the Cache Creek series or formation are separately referred to.

It thus appears that the name in question has been consistently applied by the Geological Survey of Canada to the same terrane since 1873. Nor is it merely a 'horizon' of the Carboniferous, but a formation estimated at more than 9,000 feet in thickness. It includes, in

fact, the Carboniferous formation in so far as this has been recognized in the interior district of British Columbia, and is the local representative of that formation.

GEORGE M. DAWSON.

GEOLOGICAL SURVEY OF CANADA,
April 10, 1899.

ON THE NAMES OF CERTAIN NORTH AMERICAN
FOSSIL VERTEBRATES.

THE writer, having recently had occasion to examine the literature pertaining to some of the fossil mammals of North America, has made the following notes, which he desires to record:

Hemiganus, a genus established by Professor Cope, had for its type species *H. vultuosus*. The species *H. otariidens* was described later. Dr. J. L. Wortman has, however, shown (Bull. Amer. Mus. Nat. Hist., ix., p. 167) that *H. vultuosus* is a synonym of *Psittacotherium multifragum*. The species *otariidens* is, therefore, left without generic name. I hereby propose WORTMANIA, in recognition of the valuable work which has been done by Dr. Wortman in vertebrate paleontology. The species will be *Wortmania otariidens* (Cope).

A similar case occurs among the camels. The type of the genus *Protolabis* of Cope is *P. heterodontus*. Dr. Wortman's investigation (Bull. Amer. Mus., x., p. 120) have led him to the conclusion that this so-called species is the same as the earlier described *Procamelus robustus*. The type species being removed, the remaining species requires a new generic name. I propose MIOLABIS. The type will be *M. transmontanus* (Cope).

It has also been ascertained by Dr. Wortman that the type of the genus *Systemodon*, *S. tapirinus*, is really a *Hyracotherium*, in which genus it was formerly placed. The species which have been associated with *tapirinus*, viz, *semihians*, *primævus* and *protapirinus* are, therefore, without generic name. I offer HOMOGALAX (ὁμογάλαξ, a foster brother). As type of this genus I take Dr. Wortman's *Systemodon primævus* (Bull. Amer. Mus., viii., p. 89, fig. 3).

Professor Cope has described from the Pliocene of Louisiana a fossil horse which he calls *Equus intermedius* (Proc. Amer. Phil. Soc., xxxiv., p.

463). This name has, however, been preoccupied for a quaternary horse of Europe. Trouessart (Cat. Mam., 1898, p. 794) quotes it as a synonym of *E. caballus*. The first mention I find of the name is in Rüttimeyer (Abhandl. schweiz. pal. Ges., ii., p. 24, 1877). For Professor Cope's *E. intermedius* I propose *Equus eous*.

Interea volucres Pyrois *Eous* et Aethon,
Solis equi, quartusque Phlegon, hinnitibus auras
Flammiferis implent, pedibusque repagula pulsant.
—Ovid.

Certain generic names of vertebrates have, without justice, it seems to me, been relegated to synonymy.

In 1881 Professor Cope established a genus of Condylarthra which he called *Protogonia*. Later he correctly concluded that this name had been preoccupied, probably by *Protogonius*, Hübner. He, therefore, proposed to substitute for it *Euprotogonia*, which name first appeared in a paper by Earle (Amer. Nat., 1893, p. 378, foot-note). In a recent paper Dr. Matthew (Bull. Amer. Mus., ix., p. 303) accepts this name. At the same time he shows that those remains which had originally been described by Professor Cope as *Miocænus flowerianus* belong to the earlier described *Euprotogonia puericensis*. But, for this *M. flowerianus*, Scott had in 1892 (Proc. Acad. Sci., Phila., p. 299) proposed the genus *Tetraclænodon*. The latter name, therefore, antedates *Euprotogonia* and must replace it.

In the same excellent paper (p. 268) Dr. Matthew adopts Scott's genus *Protochriacus*, founded in 1892, in preference to Cope's *Loxolophus*, proposed in 1885. The reason assigned for this preference is that Professor Cope's 'distinctions, so far as made, were based on error.' I do not believe that the best usage among naturalists at this day favors the rejection of generic names because of errors, real or supposed, in the definitions. It seems to me that *Loxolophus* must be reinstated.

With exceptions, few but important, *Oreodon* has been employed by writers for a well-known genus of Artiodactyles. Flower and Lydekker in their joint work on Mammalia use *Cotylops*, on the assumption that *Oreodon* is preoccupied by *Orodus* of Agassiz, a genus of fossil fishes. Without now discussing this conclusion, I will

recall the fact that there is a still older name which is in all respects available. This is Leidy's *Merycoidodon*, having for its type *M. culbertsoni* (Proc. Acad. Sci., Phila., 1848, p. 47). Professor Cope has rejected the name on the ground that it is a *nomen nudum*; but a generic name is hardly *nudum* when it is supported by a well-defined species and is, moreover, clothed with two pages of description.

Merycodus is another of Dr. Leidy's names which must be restored to its rightful position. This was proposed in 1854 and had for its type species *M. necatus*. On the supposition probably that this name is pre-occupied by Owen's *Merycodon*, it has been ignored. But it is incorrect to assume that any two names ending in *odus* and *odon*, but alike in other respects, clash with each other. As to their forms they are different enough to prevent confusion. As to their derivation, as has been suggested to me by my friend Dr. Leonhard Stejneger, of the U. S. National Museum, they are unlike; *odus* being the Latinized form of the Greek *ὄδος*, while *odon* comes from the Ionic *ὄδων*. The acceptance of this view will relieve us of the necessity of rejecting, on philological grounds at least, either word of many such couples as *Menodus* and *Menodon*, *Cosmodus* and *Cosmodon*.

O. P. HAY.

THE FUNDAMENTAL LAW OF TEMPERATURE FOR GASEOUS CELESTIAL BODIES.

It has been long known that an isolated celestial mass of gas rises in temperature as it radiates heat and contracts. Dr. T. J. J. See [*Astronomical Journal*, February 6, 1899; *Atlantic Monthly*, April, 1899] points out that the temperature of such a mass of gas is inversely proportional to its radius, provided the mass does not receive accretions of meteoric matter and provided the gas conforms to the laws of Boyle and Charles. When, however, the volume of the gaseous body is very great large quantities of interstellar gases and particles would fall into it and the first condition would fail; and when the gaseous body contracts to small volume it would, perhaps, be far from a perfect gas in its properties, so that the second condition would fail; to say nothing of the probable dissociation and polymerization of the

gaseous constituents due to the great changes of temperature which, no doubt, take place.

The suggestion of Dr. See that nebulous masses are extremely cold is very plausible, in view of his 'new law,' which 'may be assumed to regulate the temperature of every gaseous star in space,' but it is certainly contrary to the indications of the spectroscope; for nebulae surely are approximately in thermodynamic equilibrium in their smaller parts, if anything in the universe is; if so, there is no known agency, electrical or other, which can cause them to give off persistently abnormal radiations. Radiations (wave-length) are as intimately associated with temperature as are molecular velocities, although both may be temporarily abnormal in a given substance; for example, the velocities of the particles of a gas in a vessel may be made to deviate momentarily from Maxwell's law; a cold substance, such as calcium sulphide, may shine for a while after exposure to sunlight, and a gas in a vacuum tube may remain phosphorescent for a time as the disturbing influence of an electric discharge dies away. But it is hard to think of a certain cubic foot of nebulous matter, surrounded for millions upon millions of miles with similar matter, remote from intense radiant centers, still giving off abnormal radiations after odd millions of years. Of course, such may be the case, but Dr. See's law, in all probability, has nothing to do with nebulae at all. There is no physical reason why a nebulous mass might not be intensely hot, held together (if, indeed, we must assume it to be a gravitational unit) by the gravitation of refractory nuclei and receiving continually from space as much matter as it throws off, because of the high molecular velocity of its gaseous parts.

Dr. See's derivation of his law of temperature is incomplete and confused. It is based upon the assumption, which should be definitely proven, that the function which expresses the density in terms of the radius coordinate r remains of the same form as the external radius ρ diminishes; and he confuses *pressure per unit surface* and *pressure between given portions of matter*. Assuming the invariance of the density function Dr. See's formula may be derived as follows. Let ρ be the radius of the gaseous

mass at a given epoch. Consider the state of affairs when the radius has become $\frac{1}{2} \rho$. Gravitational forces (per unit mass) will be quadrupled and, therefore, the pressure between two contiguous portions of given mass will be quadrupled, but the area separating these portions will be quartered so that the pressure per unit area (p) will be 16 times as great. The volume v of each portion will be $\frac{1}{8}$ as great, so that pv will be twice as great. But absolute temperature is proportional to pv , therefore, the absolute temperature will have been doubled when the radius is halved. That is,

$$T = \frac{\text{constant}}{\rho}$$

"This remarkable formula," according to Dr. See, "expresses one of the most fundamental of all the laws of Nature." In simple truth it is an interesting and suggestive formula, and it may throw light upon some of the knotty questions of celestial physics.

Dr. See, in his *Atlantic Monthly* article, says among other things: "It is somewhat remarkable that, while the law of gravitation causes bodies to describe conic sections, the law of temperature for every gaseous body is represented by a rectangular hyperbola referred to its asymptotes, and thus by a particular curve of the same species." Now, it would have been quite as well, or even better, for Dr. See to have said frankly *üm-ta-ra-ra-bum-te-a*, or words to that effect; for, *seriously, the object of popular scientific writing is to develop proper and significant associations, and the bane of popular science is verbal sense which by association becomes absolute nonsense.*

In the *Astronomical Journal* for April 8th Dr. C. M. Woodward calls attention to some of the manifest inaccuracies of Dr. See's derivation of the temperature formula. He points out that the gaseous globe cannot be assumed to have a bounding surface of definite radius ρ ; he calls attention to the fact that the gravitational force at a point does not determine the pressure, but the pressure gradient at the point; and he claims that the hydrostatic pressure at a point varies inversely with ρ^2 , not with ρ^4 , as indicated in the above derivation of the temperature formula. In the above derivation, however,

the pressure is said to increase 16 times, not at the same point in space, but at a point one-half as far from the center.

The objections raised by Dr. Woodward seem to be removed as follows: Consider the gaseous mass at the epoch t . Assume that during the contraction the radius coordinate of every particle decreases in the same proportion (this is what is meant in the above discussion by the invariance of the density function.) Consider the gaseous mass at a subsequent epoch t' when the radius coordinate of every particle has been reduced to one-half its initial value. The density at a distance $\frac{1}{2}r$ from the center at epoch t' is eight times as great as at distance r from the center at epoch t , and the gravitational force is four times as great. Therefore, the weight per unit volume is thirty-two times as great, and this weight per unit volume is the pressure gradient. In integrating the pressure gradients at epoch t and t' , respectively, imagine the paths of integration to be broken up into homologous elements. The elements at epoch t' are then half as long as at epoch t , and, therefore, the integral at epoch t' from infinity to $\frac{1}{2}r$ is sixteen times as great as the integral at epoch t from infinity to r . Therefore, the pressure at homologous points is increased sixteen times when the mass of gas has contracted to half its initial dimensions, as stated in the above derivation.

W. S. FRANKLIN.

NOTES ON INORGANIC CHEMISTRY.

AN attempt is described in the *Chemiker Zeitung*, by Johann Walter, to concentrate solutions by means of a centrifugal apparatus. But while even very light and finely divided precipitates are rapidly separated by centrifugal force, an examination of different portions of a solution, taken while the machine was in rapid motion, showed that the composition was constant. The same was found true in the case of gaseous mixtures, no tendency being found for the denser constituent to collect in the most rapidly rotating portion of the vessel. This affords an interesting experimental confirmation of what might have been theoretically expected from the laws of gases and of solutions.

THE heat of formation of anhydrous oxid of

calcium has lately been redetermined by Henri Moissan from the action of water on crystallized metallic calcium. The value was found to be $\text{Ca} + \text{O} = + 145$ cal. This value is greater than that for the oxids of potassium (+ 98.2) and sodium (+ 100.9), from which it appears that calcium can replace these metals in their oxids. It is also slightly greater than that of the oxid of lithium (+ 141.2). Corresponding to this, metallic lithium was obtained by heating the oxid with metallic calcium at a red heat. The heat of formation of magnesium oxid as found by Thomsen is + 143.4, but the previous observations of Winkler were confirmed, that at a low red heat calcium is freed from its oxid by magnesium. It is suggested, therefore, that the observation of Thomsen is erroneous, owing to impurities present in the metal used.

It is interesting to find a paper from a Spanish chemist in a recent *Comptes Rendus*. J. R. Mourelo, of Madrid, describes the preparation of phosphorescent strontium sulfid from the carbonate. Finely powdered strontianite and sulfur were heated in boats in a porcelain tube while a current of nitrogen was passing. In no case was a crystalline sulfid obtained. If the strontium carbonate was pure, especially free from alkalies, the sulfid was not phosphorescent. If the temperature was too high (above a bright red heat), or if the nitrogen current was too rapid, the same was the case. The best results were obtained by using a strontianite which contained 96.12% strontium carbonate, 2.03% calcium carbonate and traces of water, manganese and iron. Particularly are the traces of manganese necessary if the strontium sulfid is to be highly phosphorescent.

A STUDY of aluminum has been made by P. Degener as to its use for culinary utensils, and published in the *Hygienische Rundschau*. While aluminum is but slightly acted on by weak acids when they are pure, in the presence of sodium chlorid it is rapidly attacked, as, for example, by sulfur dioxid, acetic acid, and even by alum. The inference is that some considerable danger attends the use of aluminum vessels in the preparation of many kinds of food. Whether, as a matter of fact, the amount which would be dissolved would do injury in the sys-

tem remains a mooted question. While many experiments seem to indicate that aluminum salts have a somewhat detrimental effect upon digestion, yet it is well known that the inhibition of large quantities of alum water is often found very beneficial to health, and many alum springs enjoy a high reputation.

J. L. H.

THE NAPLES ZOOLOGICAL STATION.

WE have recently received from Professor Anton Dohrn, the Director of the Zoological Station at Naples, a complete list of the American biologists who have worked at various times at the Naples Zoological Station. It is probable that the future demands upon the Naples tables will be quite as great as the present and the past, and the three tables, or rather two and one-half tables, which are now supported by subscriptions from this country, should be continued. Professor Dohrn has never raised any technical question of rights, but has always welcomed every American investigator. The least we can do in return is to extend to his institution the strongest support.

The Americans who have worked in the Zoological Station, the Tables they have occupied and the periods during which they were in attendance are as follows:

<i>Zoological Station.</i>			
Professor Whitman, Boston...	12 11 81	2 5 82	
Miss O'Neill, H.....	11 3 98	22 4 98	
<i>Austria.</i>			
Dr. H. K. Corning.....	6 4 92	10 5 92	
<i>Baden.</i>			
Mr. H. B. Ward, Troy.....	7 3 90	8 4 90	
<i>Bavaria.</i>			
Dr. B. Sharpe.....	19 3 83	26 5 83	
Dr. B. Dean, Columbia.....	27 4 92	3 6 92	
<i>British Association.</i>			
Dr. N. Cobb, Spencer, Mass...	11 11 88	27 1 89	
<i>Cambridge.</i>			
Miss E. A. Nunn.....	22 11 82	1 5 83	
<i>Hamburg</i>			
Dr. W. W. Norman, Ind....	5 10 89	21 3 90	
<i>Williams College.</i>			
Prof. E. B. Wilson, Baltimore.	30 3 83	20 10 83	
Prof. S. F. Clarke, Williams-town	8 1 84	1 5 92	

University of Pennsylvania and Zoological Station

Dr. Ch. Dolley, Rochester....	15	1	85	18	6	85
Dr. W. Patten, Boston (Zool.						
St. Table, 14/4/85 18/6/85..	14	4	85	23	4	85

Davis Table.

Dr. H. I. L. Russell.....	20	3	91	6	7	91
Miss I. B. Platt, Boston	7	4	91	2	7	91
Prof. E. B. Wilson, Phila....	11	1	92	1	7	92
Dr. I. W. Field, Baltimore ..	5	10	92	29	3	93
Dr. G. H. Parker, Cambridge,						
Mass	10	3	93	1	6	93
Prof. C. W. Hargitt, Syracuse	10	3	93	1	6	93
Prof. J. Gardiner, Boulder....	1	10	94	1	3	95
Dr. Ida Hyde, Chicago.....	12	3	96	1	5	96

Smithsonian Institution.

Dr. G. Fairchild, Washington.	16	11	93	17	3	93
Dr. W. H. Wheeler, Chicago..	30	12	93	14	4	94
Prof. H. C. Bumpus, Providence	25	1	94	2	4	94
Dr. L. Murbach, Berkey, Ohio.	25	4	94	23	6	96
Prof. T. H. Morgan, Bryn						
Mawr	29	10	94	15	7	95
Prof. Herb. Osborne, Ames ...	15	12	94	7	3	95
Mr. W. T. Swingle, Washing-						
ton	10	3	96	30	5	96
Dr. MacFarland, California..	11	3	96	24	3	96
Prof. F. H. Herrick, Cleveland,						
Ohio	6	11	96	4	12	96
Dr. E. Meek, Washington	19	3	97	5	5	97
Dr. H. Jennings, Michigan....	10	4	97	25	6	97
Dr. H. Neal, Cambridge, Mass.	16	4	97	25	5	97
Mr. B. M. Davis, Chicago....	29	10	97	3	12	97
Prof. H. W. Conn, Brooklyn..	11	3	98	24	4	98
Prof. D. Mottier, Indiana Univ.	12	3	98	18	4	98
Mr. W. T. Swingle, Washington.	22	3	98	28	4	98
Dr. J. R. Gerould, Dartmouth.	3	11	98			

Harvard College.

Mr. E. Rice, Middletown....	23	3	94	12	5	94
Dr. C. Child, Chicago.....	4	6	94	6	12	94
Prof. W. E. Ritter, Berkeley..	14	9	94	29	12	94
Prof. J. Reighard, Michigan..	2	4	95	3	6	95
Prof. C. C. Nutting, Iowa.....	1	6	95	17	8	95
Dr. R. T. Harrison, Baltimore.	1	2	96	1	5	96
Dr. R. C. Coe, New Haven....	17	3	96	6	5	96
Dr. A. Weyssse, Boston.....	19	3	96	18	6	96

Columbia University, One-Half Year (Resp. University Table).

Dr. A. Matthews, New York..	19	3	96	28	6	96
Dr. J. Graham, New York....	9	4	97	14	6	97
Dr. E. O. Hovey, New York..	3	11	97	4	12	97

Woman's College Table.

Prof. Miss M. Willcox, Welles-						
ley College.....	9	4	98	20	5	98
Miss Peebles Florence.....	2	9	98	19	11	98

The three tables now being supported in this country are as follows :

Smithsonian Table.—Applications should be addressed to Professor S. P. Langley, Smithsonian Institution, Washington, D. C.

University Table.—The main subscription is by Wm. E. Dodge, Esq., of New York, in the name of Columbia University. The American Society of Naturalists has also subscribed \$50 towards this table for the year 1899. Applications should be addressed to Professor T. H. Morgan, Bryn Mawr, Pa.

Women's College Table.—Supported by subscriptions from colleges, associations and private individuals.

Applications should be sent to Miss Ida H. Hyde, 91 Langdon St., Cambridge, Mass.

Students and investigators intending to visit the Station should apply to Dr. Anton Dohrn for a printed circular giving them all the necessary information as to preparation and the procedure to be observed on arrival.

SCIENTIFIC NOTES AND NEWS.

At a recent meeting of the Board of Trustees of the University of Pennsylvania the Provost was authorized to extend an invitation to the American Association for the Advancement of Science to hold its meeting in 1900 at the University.

THE medical department of Johns Hopkins University has sent a party to Manila to study the tropical diseases prevalent there in the hot season. The party includes Dr. Simon Flexner, recently elected professor of pathology in the University of Pennsylvania, and Dr. L. F. Barker, associate professor of anatomy at Johns Hopkins University.

THE field work of the United States Biological Survey during the present season will be mainly in Texas and California. Vernon Bailey, chief field naturalist of the Survey, has begun work on the coast of Texas, and will work westerly to and across the Staked Plains. Later he will join Dr. Merriam in California.

Nature states that Mr. J. Stanley Gardiner, Balfour student of the University of Cambridge, and Mr. L. Borradaile have gone to the Island of Minikoi, situated between the Maldive and

Laccadive Islands, to study the formation of coral reefs, with special reference to the depth at which the reef-building coral organisms live, the food of the coral polyps, the influence of currents upon coral formations and upon the distribution of life near them, and the inter-relationship existing between the various organisms which occur on a coral reef. It is also proposed to survey the Maldivé Islands, with a view to obtaining information as to their mode of formation. Mr. C. F. Cooper will join the expedition during the summer.

PROFESSOR T. E. THORPE has been elected to succeed Professor Dewar as President of the Chemical Society, London, while Professor W. A. Tilden succeeds Professor Thorpe as treasurer. Dr. A. Scott has been elected one of the secretaries.

THE Seventh Dutch Scientific and Medical Congress opened its sessions at Harlem on April 7th. Professor Ramsay made an address before the Section of Chemistry on 'The New Elements.'

THE first *conversazione* of the Royal Society will be held at Burlington House on Wednesday, May 3d, at 9 p. m.

It is proposed to erect a memorial statue of Sir Thomas Browne in Norwich, where the author of the *Religio Medici* practised as a physician for forty-six years. It is estimated that the statue will cost about £2,000, towards which the sum of £200 has been subscribed.

A PLAN has been proposed for erecting a monument to Dr. Jean Hemeau, of La Test, who is said to have discovered and applied the principles of microbic disease forty years before Pasteur.

THE death is announced of Dr. Franz von Hauer, formerly head of the Austrian Geological Survey, at Vienna, aged seventy-three years; of Dr. Max Durand-Fardel, President of the French Society of Hydrology, and of the Hon. F. F. Thompson, of New York, who gave Williams College scientific laboratories costing \$180,000, and generous gifts to other educational institutions.

WE regret also to record the death of Dr. George Henry Rohé, of Maryland, at New

Albany, La., while in attendance at the recent National Prison Congress. Dr. Rohé was at the time of his death President of the American Public Health Association.

THE death, at the age of 81 years, occurred on April 7th, of Mr. Joseph Stevens, the well-known geologist and antiquarian. Though a practising physician, he found time to make discoveries of neolithic and paleolithic implements and fossils, many of which are deposited in the Reading Museum, of which he was long honorary curator. He was the author of numerous publications on anthropological and archaeological subjects.

MISS E. BROWN, to whose death we recently referred, has bequeathed one of her observatories with all the contents, and, in addition, £1,000, to the British Astronomical Association. Miss Brown was Director of the Solar Section of the Association.

THE Barnard Botanical Club will give an exhibit of the work of the department of botany on the afternoon of April 28th. It is hoped that at that time the bronze tablet, given by the Club in memory of the late Dr. Gregory, will be in place. It bears the following inscription: "This laboratory, for the study of physiological botany, is dedicated to the memory of Emily L. Gregory, Ph.D., first professor of botany in Barnard College, from its opening, in 1889, until her death, in 1897."

MR. W. S. LEAN has bequeathed £50,000 to the British Museum for the extension of the library and reading room.

By the will of the late Sir William Jenner, £10,000 is bequeathed to the Royal College of Physicians of London.

THE Hon. Stevens Salisbury has presented to the Worcester Natural History Society the collection of minerals and fossils made by Mr. John Gilman.

ARRANGEMENTS have been made for the establishment of an anthropological museum at the University of Aberdeen. Several collections have already been presented to the University.

A SUBSCRIPTION has been opened in Scotland for erecting a stone over the tomb of Professor

Macgillivray, the ornithologist, in the New Calton cemetery, Edinburgh, and for founding a Macgillivray gold medal in Aberdeen University as a prize to the best student in zoology, botany or geology.

It is stated in *Nature* that some recognition will shortly be made of the services rendered to geological science by the Rev. Thomas Wiltshire, professor emeritus of geology in King's College, London. Of late years Mr. Wiltshire's labors have not been of a nature to bring his name prominently before the public, but he has been toiling quietly as the honorary Secretary and Editor of the Paleontographical Society. That Society has now published fifty-two quarto annual volumes, and some thirty of these have been edited by Mr. Wiltshire. These volumes each contain forty or fifty plates of fossils, and two hundred or more pages of letter press, dealing with organic remains of all classes. Great credit is due to Mr. Wiltshire, and the members of the Paleontographical Society (of which Dr. Henry Woodward, F.R.S., is President, and Mr. R. Etheridge, F.R.S., Treasurer) have decided to present him with a testimonial, towards which subscriptions (not limited to members of the Society) are now being received.

It is stated that the French authorities are so gratified with the success of the wireless telegraphy demonstrations between Boulogne and the South Foreland that an attempt to telegraph from Paris is proposed, and that the Eiffel Tower will be the French terminal. The English terminal will remain at the South Foreland. The direct distance between the two points is about 230 miles.

We have received the first part of the first volume of the Proceedings of the Washington Academy of Sciences issued on April 14, 1899. It consists of the first annual report of the Secretary, Mr. G. K. Gilbert. This is an interesting account of the foundation of the Academy, including the events antecedent to its formation, most of which have been recorded in this JOURNAL. It is said that the Proceedings will be continued with the publication of scientific papers.

THE Geological Society of Washington has issued the address of the retiring President, Mr. Arnold Hague, on 'Early Tertiary Vol-

canoes of the Absaroka Range,' originally published in this JOURNAL, together with an abstract of the minutes of the Society for the years 1897 and 1898. In 1898 forty-one papers were presented, the average attendance at the meetings being thirty-five. The present officers of the Society are: President, Whitman Cross; Vice-Presidents, J. S. Diller, C. W. Hayes; Treasurer, M. R. Campbell; Secretaries, T. W. Stanton, David White; Members-at-Large of the Council, S. F. Emmons, Geo. P. Merrill, Bailey Willis, N. H. Darton, A. H. Brooks.

LORD KELVIN has just prepared a report on some interesting investigations made by Professor Archibald Barr and himself in Edinburgh, Bradford and Oldham on the subject of the destruction of town refuse. According to the *London Times* the report is not only of great interest to local authorities, but to the general public. In one instance he experimented on damp ashpit refuse containing a large proportion of night soil and vegetable matter from markets and shops. This was consumed without the slightest trace of smoke. In addition to this solution of the smoke difficulty the residual products proved to be of great commercial value. In another case the steam produced by the process of destruction was utilized for the driving of electric lighting machinery and other power purposes. No coal or coke whatever was employed, and in this instance also there was an entire absence of smoke. Lord Kelvin's report demonstrates that public bodies have no longer any excuse for referring to 'waste products,' but have within their reach the means of turning the most unpromising kinds of refuse to a highly profitable account.

THE Twenty-Seventh Annual Meeting of the American Public Health Association will be held at Minneapolis, Minn., beginning October 31st, and continuing until November 4, 1899. The Executive Committee has selected the following topics for consideration: (1) The Pollution of Water Supplies; (2) The Disposal of Garbage and Refuse; (3) Animal Diseases and Animal Food; (4) Car Sanitation; (5) Steamship and Steamboat Sanitation; (6) The Eti-

ology of Yellow Fever; (7) The Relation of Forestry to the Public Health; (8) Demography and Statistics in their Sanitary Relations; (9) The Causes and Prevention of Infectious Diseases; (10) Public Health Legislation; (11) The Cause and Prevention of Infant Mortality; (12) The Period during which Each Contagious Disease is Transmissible and the Length of Time for which each Patient is Dangerous to the Community; (13) Sanitation, with special reference to Drainage, Plumbing and Ventilation of Public and Private Buildings; (14) Method of International Arrangement for Protection against the Transmission of Infectious Diseases; (15) Disinfectants; (16) To Examine into the existing Sanitary Municipal Organizations of the Countries belonging to the Association with a view to Report upon those most successful in Practical Results; (17) Laboratories; (18) To define What Constitutes an Epidemic; (19) National Leper Home; (20) Revision of Classification of Diseases; (21) Dangers to the Public Health from Illuminating Gas Leakage.

A CORRESPONDENT of the London *Times* calls attention to a passage in *The Spectator* (No. 241, 1711) which is interesting in connection with wireless telegraphy and telegraphy in general. The passage read thus: "Strada in one of his Prolusions gives an account of a chimerical correspondence between two friends by the help of a certain loadstone, which had such virtue in it that if it touched two several needles, when one of the needles so touched began to move, the other, though at never so great a distance, moved at the same time and in the same manner. He tells us that the two friends, being each of them possessed of one of these needles, made a kind of dial-plate, inscribing it with the four and-twenty letters in the same manner as the hours of the day are marked upon the ordinary dial-plate. They then fixed one of the needles on each of these plates in such manner that it could move round without impediment so as to touch any of the four-and-twenty letters. Upon their separating from one another into distant countries they agreed to withdraw themselves punctually into their closets at a certain hour of the day and to converse with one another by means of this their invention. Accordingly when they were

some hundred miles asunder each of them shut himself up in his closet at the time appointed, and immediately cast his eye upon his dial-plate. If he had a mind to write anything to his friend he directed his needle to every letter that formed the words which he had occasion for, making a little pause at the end of every word or sentence to avoid confusion. The friend, in the meanwhile, saw his own sympathetic needle moving of itself to every letter which that of his correspondent pointed at. By this means they talked together across a whole continent, and conveyed their thoughts to one another in an instant over cities or mountains, seas or deserts."

UNIVERSITY AND EDUCATIONAL NEWS.

IN its session just closed the Legislature of Nebraska made provision for the University of Nebraska for the biennium ending March 31, 1901, as follows: University salaries, \$230,000; University expenses (including U. S. funds for agricultural and mechanic arts), \$172,500; buildings and other improvements, \$93,500.

THE Queen has appointed the Earl of Kimberley, K.G., to be Chancellor of the University of London, in lieu of the late Lord Herschell.

THE University of Chicago has awarded eighty-one fellowships, of which the following are given in the sciences: *mathematics*, G. A. Bliss, H. Lloyd, W. Findlay, D. N. Lehmer, J. H. MacDonald; *astronomy*, C. E. Rood, W. S. Adams, A. C. Lunn; *physics*, H. O. Murfee, R. F. Earhart, C. W. Chamberlain, F. Reichmann; *chemistry*, H. E. Goldberg, W. McCracken, M. D. Slimmer, S. F. Acree; *geology*, W. W. Atwood, W. N. Logan, R. George, W. T. Lee, W. G. Tigt; *zoology*, H. E. Davies, R. S. Lillie, F. M. Guyer, H. H. Newman; *botany*, A. C. Moore, B. E. Livingston, S. M. Coulter, F. M. Lyon; *physiology*, R. R. Rogers, W. E. Garrey, R. W. Webster; *neurology*, D. M. Shoemaker; *sociology*, R. G. Kimble, A. T. Freeman, A. D. Sorenson; *anthropology*, A. W. Dunn; *pedagogy*, W. A. Clark; *philosophy and psychology*, H. W. Stuart, H. B. Thompson, R. L. Kelly, H. H. Bawdin.